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THE INFLUENCE OF SCIENCE UPON MODERN CULTURE

Conference Commemorating the 400th Anniversary of the Birth of Francis Bacon

Sponsored Jointly by
The American Philosophical Society
and
The University of Pennsylvania
January 23–24, 1961

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PAGE

Introduction. Roy F. Nichols 459

The Program. Murray G. Murphey 461

Cosmos and Creation: Science and Theology in Reformation Thought. Jaroslav Pelikan 464

Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science. Robert K. Merton 470

Bacon and Modern Physics: A Confrontation. Henry Margenau 487

Science as a Form of Systematic Knowledge: Some Contributions from Mathematics. Carroll W. Newsom 493

Science as a Social Phenomenon. Robert M. MacIver 500

Science as Technology. Lewis Mumford 506

The History of Science in American Universities. Richard H. Shryock .. 512

THE INFLUENCE OF SCIENCE UPON MODERN CULTURE: INTRODUCTION *

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THE historian finds in the course of his searches certain implications in human behavior which lead him to generalize. One of his generalizations is that he observes in man a factor of confusion arising from his physical mechanism. Part of this mechanism is a system, or lack of system, of unconscious action or impulse with which a man is born and another element is an operation of control which man has organized, a reasoning machine. Much of the life of man, and consequently the life of society, is the result of the fact that these two mechanical elements directing man's behavior often work at cross purposes and produce confusion. Often the impulse directs, sometimes the reason controls, but much of the time there is a debilitating and frustrating warfare between the two that produces stalemates and dilemmas best known to psychiatry.

It is a part of prevailing folklore that this is an age of science which implies that reason has triumphed, but it is all too clear that science, which in essence is order, has not impressed itself too deeply upon human conduct as today it is observed. Man stands in a highly emotional state, with his finger nervously fingering the trigger of an instrument capable of his own annihilation.

Under today's circumstances, what should a Philosophical Society and a University have in their programs to promote useful knowledge about science so that its function and its limitations in society may be better understood? Everyone is agreed that science should be taught as expertly as possible and that scientific research should be promoted to the limit. But when it comes to the question of what society should know about science and its functions there is uncertainty. Should there be educational programs in the history of science, or in the social functioning of science, or in the philosophy of science? What is the responsibility of a learned society and a university in this domain?

There could probably be no time more appropriate for the consideration of these implications than a commemoration of the birth of Francis Bacon, one of the pioneers, not only in scientific experiment, but in thinking about science. In view of these circumstances, representatives of the American Philosophical Society and the University of Pennsylvania took the occasion of the four hundredth anniversary of the birth of Francis Bacon as an opportunity to organize a conference designed to explore further the nature of science, and its meaning in the life of man and the problem of communicating scientific findings more extensively and more intelligibly to society. It is not possible that man knows too much science but it is probable that he knows too little about its nature and its social significance; about its influence on human behavior, both in thought and action.

Six papers were delivered and discussed at the two sessions. They are published on the pages which follow. At the commemoration dinner, held in the midst of the conference, Dr. Loren C. Eiseley, Provost of the University, delivered an eloquent address: "Francis Bacon as an Educator," in which he maintained that Bacon has on occasion been hardly dealt with. The philosopher was dedicated to discovering better means of understanding the universe and to that end he believed that striving toward the light of reason was the great imperative. He sought to educate all conditions of men at various levels of capacity to make discoveries, great and small. By this wholesale, many-leveled induction, over weary centuries, Bacon believed that men might eventually discover the "beautiful, dry light of reason," endow it with a content of charity and "by greater art than now we practice," make "our peace with the nature of things." Four hundred years is too short a time and man's progress in those centuries too limited to make it yet possible to render balanced justice to this great educator

*Contribution to the Conference Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored jointly by the American Philosophical Society and the University of Pennsylvania, January 23–24, 1961.

whose purpose is still to be achieved.[1] At the conclusion of the formal sessions, a group of teachers of the history of science held an informal discussion under the leadership of Dr. Richard H. Shryock.

The Conference planners thus attempted to approach these problems in the spirit of Francis Bacon, bearing in mind his distinction between the world of notions and the world of facts and recognizing that mankind must perforce live in both worlds.[2] It is to be hoped that these conferences have formulated some useful concepts which can now be transmitted to a puzzled and confused society, concepts which will come most appropriately from a philosophical society and a university working in cooperation.

[1] This address was published in *Science* 133 (3460): 1197–1201, April 21, 1961.

[2] These conferences were held at the University of Pennsylvania on January 23, 1961, and at the American Philosophical Society on January 24. Dr. Henry Allen Moe, Dr. George W. Corner, Dr. Roy F. Nichols, and Dr. Richard H. Shryock presided at the several sessions. The Rapporteur was Dr. Murray G. Murphey, Associate Professor of American Civilization at the University of Pennsylvania.

THE INFLUENCE OF SCIENCE UPON MODERN CULTURE: THE PROGRAM *

MURRAY G. MURPHEY

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AMONG the founders of modern science Francis Bacon holds a position which is unique. He made no great discovery; he formulated no great theory; there is no Bacon's Law or Bacon's Theorem. Yet his influence upon the history of science has been incalculable. For it was Bacon who first looked beyond the particular results of science and grasped the fact that it was in the method of reaching those results that the essence of science lay. Bacon's *Novum Organum* was the first great treatise on the inductive method, and whatever its shortcomings it directed men along the road of the future. Moreover, this concern with method and procedure led Bacon to attend to aspects of science which others largely ignored. He was one of the first to inquire into the institutional aspects of science and the likely means of promoting its practice. And he saw with unexampled keenness the immense technological potential of the new learning. This breadth of view, combined with the methodologist's concern for future results, and with a naturally sanguine temperament, made Bacon not only a student of science but its prophet as well, proclaiming in the wilderness a new birth of time. Now, on the four hundredth anniversary of his birth, it is but just that we should measure his words against what time has born, and to this end we have brought together a group of distinguished scholars.

We think of Bacon as the herald of our own era. Yet he spoke in an age still medieval, when men's thoughts were on another world and when the glory of the race lay in the times lost. When Bacon was born, Calvin was still alive, and Luther but fifteen years dead. The very year the *Novum Organum* was published, the Pilgrims fled to New England to try to recreate the primitive church. How vast this vision of lost glory loomed, Professor Pelikan has shown us in his analysis of Luther's *Lectures on Genesis*, and it is only in terms of this fabulous picture of what man had been, and the hideous reality of what he had become, that one can understand why it was necessary for Bacon to inveigh against those who held the wits of ruined man too weak to pry out nature's secrets. And it is only in terms of this historical perspective that one can fully understand Bacon's own concept of the mission of science. For Bacon wrote:

> We copy the sin of our first parents while we suffer for it. They wished to be like God, but their posterity wish to be even greater. For we create worlds, we direct and domineer over nature, we will have it that all things *are* as in our folly we think they *should be*, not as seems fittest to the Divine wisdom, or as they are found to be in fact; and I know not whether we more distort the facts of nature or of our own wits; but we clearly impress the stamp of our own image on the creatures and works of God, instead of carefully examining and recognizing in them the stamp of the Creator himself. Wherefore our dominion over creatures is a second time forfeited, not undeservedly; and whereas after the fall of man some power over the resistence of creatures was still left to him—the power of subduing and managing them by true and solid arts—yet this too through our insolence, and because we desire to be like God and to follow the dictates of our own reason, we in great part lose.
>
> If, therefore, there be any humility towards the Creator, any reverence for, or disposition to magnify His works, any charity for man and anxiety to relieve his sorrows and necessities, any love of truth in nature, any hatred of darkness, any desire for the purification of the understanding, we must entreat men again and again to discard, or at least banish for a while, those volatile and preposterous philosophies which have preferred theses to hypotheses, led experience captive, and triumphed over the works of God. Drawing nigh in all humility and reverence they must unroll the volume of Creation; thereon must they dwell and meditate; this must they pursue purely and sincerely with minds washed clean from preconceived opinions. For this is that sound and language which "went forth into all lands," and did not incur the confusion of Babel; this should men study to be perfect in, and, becoming again as little children, condescend to take the alphabet of it into their hands, and spare no pains to search and unravel the interpretation thereof, but pursue it strenuously and persevere even unto death.[1]

*Contribution to the Conference Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored jointly by the American Philosophical Society and the University of Pennsylvania, January 23–24, 1961.

[1] Abbott, Edwin A., *Francis Bacon, an account of his life and works*, 411, London, Macmillan, 1885.

For Bacon science and faith were alike given by God to help men regain some semblence of Adam's high estate. The mirror of the mind must be cleansed of its vanities and made to reflect the true divine image. "For we are founding a real model of the world in the understanding, such as it is found to be, not such as man's reason has distorted."[2]

The problem to which Bacon addressed himself in the *Novum Organum* was that of finding a method by which such a "real model of the world" could be constructed. And the exposition of this method which Bacon gave was in its day, and yet remains, one of the classic expositions of scientific method. We must begin, he argued, with careful, systematic observation and experiment, and the results of these observations and experiments must be accurately and systematically recorded in a "Natural History." When a store of such data has been gathered, it must then be so organized and employed as to permit the "induction" of an explaining generalization. Although for lack of better terms Bacon called these generalizations "forms," it is perfectly clear that he meant by this a general law, or statement of a constant connection in experience between the phenomenon investigated and some other observable or observables.[3] In order to reach such generalizations, Bacon introduced his justly famous tables, in which he formulated for the first time in the literature of science the methods of resemblance, difference, and concomitant variation. (These names were given by Mill, but the methods are Bacon's.) And in his use of these tables, Bacon showed a much keener insight into the nature of science than have most of his critics. Bacon perfectly understood that a law is not a mere summary of particulars: such pure empiricism he regarded as useless. "The true labor of philosophy . . . neither relies entirely nor principally on the powers of the mind, nor yet lays up in the memory the matter afforded by the experiments of natural history and mechanics in its raw state, but changes and works it in the understanding."[4] Similarly, for simple induction by enumeration of positive instances, Bacon had nothing but contempt: such induction he called "puerile."[5] "The first work of legitimate induction, in the discovery of forms, is rejection . . . ,"[6] wrote Bacon, thereby showing that he understood perfectly that the primary function of the tables was to disprove alternative hypotheses. And having arrived by these means at an hypothesis which stands the test of our present and incomplete data, we may adopt it for now as a tentative hypothesis, or "first vintage,"[7] but such an hypothesis must then be further tested and confirmed by new experiments and observations.[8] In other words, what Bacon is describing here, however imperfectly, is the hypothetico-deductive-inductive method which has been the basis for scientific procedure from his day to ours.

Although Bacon understood the essentials of the scientific method, his formulation of it, as Professor Margenau has noted in some detail, would not have permitted the construction of the more powerful models employed in modern physics. For in such cases—e.g. in relativity theory—the choice of the best model is influenced in essential ways by factors which must be regarded as contributions of our minds. And as President Newsom has shown in a lucid paper on the development of modern scientific systems, it follows from this that Bacon's phrase "real model of the world" may have no meaning, for it now seems impossible to distinguish the contribution made by our own minds from what is given in experience. Thus that same presumption of our minds against which Bacon inveighed now appears to be an essential constituent of the very method which he championed.

Having defined the aim of science as the discovery of explanatory laws, Bacon recognized that to explain a phenomenon is to show how to produce it. And this insight led at once to the further recognition that the growth of the sciences must increase man's dominion over nature and so permit a revolution in the material basis of life. In science, therefore, "Truth . . . and utility, are here perfectly identical . . . ,"[9] and the accurate description of the Divine Ideas[10] implies "the endowment of human life with new inventions and riches."[11] Indeed, it is one of the most remarkable facts about this Lord Chancellor of England that he should have been so completely

[2] Bacon, Francis, *Advancement of learning and novum organum*, Intro. James Creighton, 363, New York, Willey Book Co., 1944.
[3] *Ibid.*, 322, 288.
[4] *Ibid.*, 349.
[5] *Ibid.*, 353.
[6] *Ibid.*, 388.
[7] *Ibid.*, 390.
[8] *Ibid.*, 353, 358.
[9] *Ibid.*, 363.
[10] *Ibid.*, 317.
[11] *Ibid.*, 339.

free from any contempt for the mechanic arts that he counted "the introduction of great inventions . . . one of the most distinguished of human actions"[12] and gloried in the vision of a New Atlantis replete with technological devices and material goods far beyond the dreams of Elizabethan England. And here again time has fulfilled Bacon's prediction, though not, Professor Mumford holds, in quite the form Bacon intended, or desired. For what distinguished the men of New Atlantis was not, Professor Mumford notes, just that they had better machines, but that they were better Christians, and it may be doubted whether that complete reciprocity of mechanical power and spiritual insight which Bacon envisaged has been realized in our day.

Among the pioneers of science, Bacon possessed the keenest insight into the institutional aspects of science. For Bacon did not consider scientific progress inevitable except under certain conditions, and these conditions are to a considerable extent social. Thus he notes, "For out of the twenty-five centuries, with which the memory and learning of man are conversant, scarcely six can be set apart and selected as fertile in science and favorable to its progress."[13] Only in Greece, Rome, and western Europe, and there for only two centuries apiece, has science flourished. To explain this phenomenon, Bacon points to the level and nature of education, the amount of leisure, the degree of specialization, the attitudes toward manual labor, the ethos of the society, the availability of written records, and the patronage of the prince—all of them sociological factors. As Professor MacIver has noted in developing this theme, "In every society, including our own, the reach of scientific development is determined by a complex of social and environmental conditions." Bacon made no attempt, as sociologists such as Professor MacIver have since made, to state generalizations explaining the differential scientific productivity of societies, but he did utilize this sociological insight in formulating a program for the advancement of science. For as every historian of science knows, Bacon's description of Solomon's House in the *New Atlantis* was one of the chief inspirations for the founding of the Royal Society.[14]

But of all Bacon's doctrines, the one which has produced the greatest controversy over these past four centuries is the thesis that given the proper conditions and the proper method, scientific discovery is inevitable. Taken in its extreme form, Bacon's assertion that "Our method of discovering the sciences is such as to leave little to the acuteness and strength of wit, and indeed rather to level wit and intellect."[15] has been interpreted to mean that it is possible to induce from a set of particulars a true, explaining generalization by a procedure which is purely mechanical. Philosophers, scientists, and methodologists have ever since derided Bacon's naiveté and have pointed out repeatedly that neither his, nor any known method, is capable of such a feat. Yet there is something here that will not down. For if the philosophers are right, and true hypotheses are, as Einstein said, free creations of the mind, how does it happen that simultaneous discovery so often occurs? Shakespeare's *Hamlet* would not have been written had Shakespeare never lived, yet there is some sense in which most of us believe that relativity would have been discovered had Einstein never lived. To this fundamental question, Professor Merton has addressed himself in an extraordinary paper on single and multiple discovery—a paper which takes a long step toward the development of a sociological theory of scientific discovery. And it is perhaps the most fitting tribute of all that on this four hundredth anniversary of Bacon's birth, the method of the *Novum Organum* should be applied to the subject of the *Novum Organum*.

[12] *Ibid.*, 365.
[13] *Ibid.*, 337.
[14] Bacon, Francis, *New Atlantis*, Intro. Alfred Gough, Oxford, Clarendon Press, 1915.
[15] Bacon, *op. cit.*, *Advancement of learning*, 326.

COSMOS AND CREATION: SCIENCE AND THEOLOGY IN REFORMATION THOUGHT*

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FRANCIS BACON was born on January 22, 1561, less than fifteen years after the death of Martin Luther, which occurred on February 18, 1546. This chronology could serve as an allegory of the great gulf fixed between what C. P. Snow has identified as "the two cultures," that of the humanistically trained literati and that of the empirical scientists. Yet there is an irony in this chronology that even Sir Charles, for all his sensitivity to the irony and pathos in intellectual history, has not described. In the thought of Luther and of his associates, Renaissance humanism and Reformation theology combined to produce a sense of liberation and novelty, at the very time when the scientific wave of the future, "far back, through creeks and inlets making," began to swell a tide that eventually swept away much of both Renaissance humanism and Reformation theology. Galileo came into conflict with the Holy Office, but Kepler's adversaries were the epigoni of the Reformers.

This essay is a study of the sources of that irony. It is based upon one of the longest and most neglected works of Reformation theology, Luther's *Lectures on Genesis*, which occupied him from 1535 to 1545 and which will comprise eight of the thirty volumes of his commentaries on the Bible in our new American Edition. Two volumes of the *Genesis* have appeared so far, under my editorship;[1] a third is scheduled for publication in the autumn of 1961. Although Luther's principal concern in these lectures is with "the history of the people of God,"[2] the two creation narratives at the very beginning of the Book of Genesis and the story of the deluge a little later do compel him to give the problems of cosmos and creation more sustained attention than he does anywhere else in his works. The manuscript of his lectures has been bowdlerized by his more orthodox disciples, who were in many ways closer to Melanchthon than to Luther; but for the purposes of this paper their editorial corrections provide further evidence of the relation between science and theology in Reformation and post-Reformation thought.[3]

When Luther took up the exposition of Genesis, he knew that he was following a long succession of exegetes from both the patristic and the medieval period who had commented upon it before him. From the vast majority of them he set himself consciously apart, on the grounds that his exegesis of the text was strictly historical; while that of the tradition had been unduly preoccupied with allegories. It was "the historical and strict meaning" of the text that he strove to discover, "the historical and literal meaning, which is in harmony with the text."[4] He read the accounts of the Book of Genesis as history, postponing allegorical interpretation until he had determined the historical sense. For allegory was to this historical sense what rhetoric was to logic; "it ought to illustrate the historical account but has no value at all for giving proof."[5] To paraphrase an old American joke, some exegetes use allegory as a drunken man uses a lamppost: it is intended for illumination, but they lean upon it for support. Luther said that as the natural color of a body surpassed even

*Read January 23, 1961, in the Conference on the Influence of Science upon Modern Culture, Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored jointly by the American Philosophical Society and the University of Pennsylvania.

[1] The Latin text of the *Enarratio in Genesin* appears in Volumes 42–44 of *D. Martin Luthers Werke* (Weimar, 1911–1915). The English translation appears in Pelikan, Jaroslav (ed.), *Luther's works*. American edition 1 (Saint Louis, 1958) and 2 (Saint Louis, 1960). References in further footnotes will be as follows: first to the Weimar Edition (abbreviated as *WA*), by volume, page, and line; then in parentheses to the American Edition (abbreviated as *AE*), by volume and page.

[2] *Cf.* Pelikan, Jaroslav, *Luther the expositor*, 89–109, Saint Louis, 1959; most of that material appeared earlier in Pelikan, Jaroslav, Die Kirche nach Luthers Genesisvorlesung in Vilmos Vajta (ed.), *Lutherforschung heute*, 102–110, Berlin, 1958.

[3] *Cf.* Meinhold, Peter, *Die Genesisvorlesung Luthers und ihre Herausgeber*, Stuttgart, 1936, and my comments, *AE* 1: xi–xii.

[4] *WA* 42: 99, 3 (*AE* 1: 132); *WA* 42: 138, 40 (*AE* 1: 185).

[5] *WA* 42: 173, 42 (*AE* 1: 233).

a beautiful painting, so the historical interpretation of a passage surpassed the allegorical interpretation.[6]

Both Luther the churchman and Luther the professor were speaking here. The recovery of the historical sense of the Scriptures was a requirement both of his Reformation theology and of his humanistic scholarship. As one of the first theologians since apostolic times to study the Old Testament in the original Hebrew, Luther made use of the best textual, lexicographical, and historical data available to him in the scholarship of men like Johannes Reuchlin, Santes Pagninus, and (through the Franciscan Nicolaus of Lyra) the medieval rabbis. Some of his textual conjectures and linguistic intuitions were exceedingly acute and have been substantiated by later research.[7] Yet he was also critical of an exegesis that neglected theology for the sake of mere philology; such an exegesis failed to grasp the historical meaning of the text.[8] When Moses spoke of six days, he must have meant six literal days. It is amusing that whereas later Biblical theologians tried to accommodate their exegesis to science by extending the six days to six epochs, Luther was concerned with the opposite extreme; he was attacking theologians like Augustine and Hilary, in whose exegesis the hexaemeron was reduced to a single instantaneous act of creation. For Luther, such exegesis was sophistry. He was so bent upon historical interpretation, in the name of both scholarship and faith, that he tried to gauge the time of day when Eve was tempted and fell, deciding that it was at noon.[9]

Applied to the narrative of the first three chapters of the Book of Genesis, this insistence upon the historical meaning of the text compelled Luther to treat the account of the state of innocence as straight prose. Repeatedly he confessed how difficult such an interpretation was—not because of the absurdities to which it led, but because "we cannot even conceive of it [the *status integritatis*] in our thinking." "Now that we have lost the state of living in innocence," he complained, "it is easier to form an opinion about that life than to give an actual description of it."[10] But he did form many opinions about it and did try to give as detailed and accurate a description of it as he could. He puzzled over the problem of the sanitary facilities in the menagerie on Noah's ark; and instead of asking the hackneyed question of where Cain got his wife, Luther expressed his admiration for a girl who would marry—or remained married to—a man guilty of so heinous a crime.[11] In the name of scientific theology, then, Luther set forth an exegesis destined to be shaken by each successive scientific discovery about both man and the world. Columbus supposed that on his third voyage he had discovered the earthly Paradise; thus "mythical geography still obsessed the man who had just opened up the way to so many real discoveries."[12] In the same fashion Martin Luther, who effected a "Copernican revolution"[13] in Christian theology, was still obsessed by the vision of a historical state of integrity, in which both man and the world lived as the will of the Creator had originally intended them to live before the fall into sin.

This obsession with a historical fall required Luther to use exaggerated language about the purity and nobility of man in the state of innocence. He had to say that the physical prowess of man then exceeded anything possible now. If man had not fallen into sin, children would not have needed to be nursed by their mothers for very long. In fact, they would have been able to stand upright immediately, as baby chicks do, and to run about. Childbirth would have been without pain, and the fertility of the human female would have been much greater; therefore conception would have been quicker and easier, with multiple births far more frequent than they are now, when even "those who are most fertile give birth at most to one child in a single year." In the state of innocence, man should have remained a vegetarian and would therefore have avoided the obesity that has begun to beset him since he became a carnivore. Since nothing in the state of integrity could have been unpleasant, he would likewise have retained a pleasant odor; man must have smelled "delicate and delightful" before he fell into sin. His power of vision then

[6] *WA* **42**: 367, 16 (*AE* **2**: 150).

[7] *Cf.* the brief comments of M. Reu, *Luther and the Scriptures*, 103–108, Columbus, Ohio, 1944.

[8] *WA* **42**: 219, 37 (*AE* **1**: 298); *WA* **42**: 272, 10 (*AE* **2**: 15 and my note there).

[9] Augustine and Hilary: *WA* **42**: 4, 26 (*AE* **1**: 4 and my notes there); *WA* **42**: 91, 22 (*AE* **1**: 121). The time of Eve's temptation: *WA* **42**: 62, 6 (*AE* **1**: 82).

[10] *WA* **42**: 99, 15 (*AE* **1**: 132); *WA* **42**: 315, 10 (*AE* **2**: 74).

[11] Noah: *WA* **42**: 309, 39 (*AE* **2**: 67); Cain: *WA* **42**: 230, 31 (*AE* **1**: 313).

[12] Eliade, Mircea, *Myths, dreams, and mysteries*, translated by Philip Mairet, 55, New York, 1960.

[13] *Cf.* Nygren, Anders, *Agape and Eros*, translated by Philip S. Watson, 681–691, Philadelphia, 1953.

must have exceeded that of the eagle now, and his physical strength must have enabled him to handle lions and bears as we handle puppies nowadays. "Man would never have experienced the inconveniences of old age; his forehead would never have developed wrinkles; and his feet, his hands, and any other part of his body would not have become weaker or more inactive." [14]

The intellectual powers of man must have been equally magnificent. Regardless of how many generations of men might have followed Adam, it would have been unnecessary to instruct them about the mystery of human origins. Not merely the transmission of this information through the sinless generations of immortal men, but the intuition by which Adam recognized Eve as bone of his bone and flesh of his flesh would have taught the descendants of Adam about their beginnings in the creating action of God.[15] In the *Lectures on Genesis* Luther—or one of his editors—was willing to say that if it had not been for the fall, Adam and his descendants would never have needed civil government, but would have lived in social peace and domestic tranquillity automatically. (Elsewhere in his writings, however, Luther indicated that even apart from sin, government was necessary as a means of ordering human life, as there was government in heaven among the holy angels, who neither have sinned nor can sin.[16]) Adam's intellectual powers before the fall entitled him to the name "philosopher." From the statement of the Book of Genesis that Adam gave names to all the animals Luther concluded that he must have been endowed with a remarkable insight into the true nature and inner disposition of the other creatures. He understood the trees and herbs, and he even had "the most dependable knowledge of the stars and of the whole of astronomy." [17]

Even more remarkable than the physical and intellectual endowments of primitive man were his moral gifts. Luther was especially impressed with the meaning of sex in the state of innocence. Ex-monk though he was, he tried to imagine sexual desire without lust. He spoke of "a transcendent decency, not shame and embarrassment" in sexual intercourse under the conditions of primitive innocence. Adam felt an "overwhelmingly passionate love," which was a "need for a delightful and full relationship or cohabitation in both love and holiness." Repeatedly Luther emphasized that in the state of integrity sexual union would have been an expression not of libido, but of obedience toward God. Like Augustine, he interpreted the involuntary nature of sexual desire as a symptom of the sin that corrupts all of life. But Luther found more positive accents than Augustine usually did for speaking about the sexual side of what Augustine called "the good of marriage." In one place he even said that sexual union would have been "most delightful" in the state of innocence because it would have been unaccompanied by shame and sin. Indeed, "after the proclamation of the name of God it [procreation] is the most important activity Adam and Eve in the state of innocence could carry on—as free from sin in doing this as they were in praising God." The symbol for this uninhibited and holy innocence was the nakedness of Adam and Eve before the fall.[18]

Nevertheless, this very effort to interpret the state of innocence as a historical reality involved Luther in the danger of making Adam and Eve so pure and noble that the historicity of the fall was difficult to maintain. How could so superior a being have been deceived by the serpent? Everything Luther said to make the fall appear serious also made it appear incredible. Therefore, he was obliged eventually to resort to a definition of the *status integritatis* as "the innocence of a child." [19] In the early Church, Irenaeus described Adam and Eve as children, and among our contemporaries Paul Tillich has spoken about "dreaming innocence" as the perennial meaning of the story of Paradise.[20] Reasoning *a posteriori* from the fall, Luther said: "I call it the innocence of a child because Adam was, so to speak, in a middle position and yet could be deceived by Satan and fall into disaster, as he did." A little later, in the

[14] Children in the state of innocence: *WA* **42**: 78, 5 (*AE* **1**: 102); *WA* **42**: 54, 37 (*AE* **1**: 72). Childbirth without pain: *WA* **42**: 79, 32 (*AE* **1**: 104); *WA* **42**: 100, 3 (*AE* **1**: 133). Fertility: *WA* **42**: 162, 6 (*AE* **1**: 217). Vegetarianism: *WA* **42**: 55, 13 (*AE* **1**: 72); *WA* **42**: 425, 24 (*AE* **2**: 231). Odor: *WA* **42**: 76, 17 (*AE* **1**: 100); *WA* **42**: 84, 10 (*AE* **1**: 110). Vision and strength: *WA* **42**: 46, 21 (*AE* **1**: 62 and my note there). Perpetual youth: *WA* **42**: 70, 22 (*AE* **1**: 92).

[15] *WA* **42**: 96, 19 (*AE* **1**: 128).

[16] *WA* **42**: 79, 7 (*AE* **1**: 104). *Cf.* George W. Forell, *Faith active in love*, 139-155, New York, 1954.

[17] *WA* **42**: 49, 39 (*AE* **1**: 66).

[18] *WA* **42**: 79, 38 (*AE* **1**: 105); *WA* **42**: 102, 27 (*AE* **1**: 136); *WA* **42**: 89, 1 (*AE* **1**: 117); *WA* **42**: 105, 3 (*AE* **1**: 139).

[19] *WA* **42**: 84, 24 (*AE* **1**: 111).

[20] On Irenaeus *cf.* Pelikan, Jaroslav, *The shape of death*, 103, New York, 1961. Tillich, Paul, *Systematic theology* **2**: 33 ff., Chicago, 1957.

very midst of an encomium on the powers of primitive man, he had to stop to clarify this point again: Adam "was righteous and upright . . . , endowed with extraordinary perception and an upright yet imperfect will. For perfection was postponed until the spiritual life after the physical one." So Luther recognized that the term "perfection" has an unavoidably teleological force, as the Greek word *teleiōsis* makes clear. Hence he was obliged to posit the thesis that even in the state of innocence this temporal life would have been followed by another and an eternal life.

From this thesis it is clear that Luther's interpretation of the meaning of death in relation to the state of innocence also had to be paradoxical. Throughout Christian history theologians have defined death as "the wages of sin." But they have also found it necessary to define it, in relation to the process of time in which life moves, as simply termination.[21] The first definition has caused them to see mortality as something unnatural, added to the human constitution because of sin but not an original part of man's created temporal existence. The second definition has come from the recognition that the idea of an infinite senescence, even the senescence of an innocent being, is an absurdity. Luther took the occasion of the story of Eve's creation to expound his notion that, after living out his intended time span, Adam would have fallen asleep among the roses—which, as we shall see, were not yet equipped with thorns—and that "during that sleep he would have been changed and transported into the spiritual life without experiencing any pain, just as he did not realize that his body was being opened and that a rib, with flesh, was being taken out."[22] Nothing so aptly illustrates the problems created by a historical exegesis of Genesis as this need to acknowledge the naturalness of death in the midst of a discussion whose primary theme was the gravity of sin and the terror of death as the wages of sin. Dying belongs to time and history, and the only way to eliminate it from the primitive state is to remove the primitive state from time and history; yet both theology and scholarship obliged Luther to immerse the primitive state in history.

Nothing less catastrophic than a fundamental transformation of the cosmos by the fall of man was the inevitable corollary of this historical exegesis. "Before sin," according to Luther, "the sun was brighter, the water purer, the trees more fruitful, and the fields more fertile." He became especially extravagant in his description of the serpent before the fall of man. It walked erect, like a rooster or a peacock. It ate fruit rather than earth, and it had neither scales nor a poisonous tail. Thus the serpent was a beautiful and friendly beast, indeed a pet. Other animals had likewise been perverted by man's sin. The very distinction between wild animals and tame animals was a result of sin; for though these animals were all created at the beginning, man's fall into sin had rendered some of them dangerous to him. About other noxious creatures, however, Luther advanced a more startling suggestion. "Darnel, wild oats, weeds, nettles, thorns, thistles . . . poisons, injurious vermin. . . . All these were brought in through sin." Toads, flies, butterflies, caterpillars, bedbugs, and the like were all the result of the fall. And if there had been no fall, there would have been perpetual spring, without snow or frost. In such a climate man would not have needed houses, but could have gone on living in the garden.[23]

Contrary to his own intention, Luther was thus compelled to posit a second creation as a consequence of the fall. Entire orders of creatures came into being not "in the beginning," but after the disobedience of Adam and Eve. One theological reason for reading the story of the creation and the story of the fall as history has been to distinguish between created existence and sin and thus to avoid the implication that time is inherently evil.[24] If the two stories of creation and the fall are not read as historical accounts, they may easily be combined into one story, whose meaning is the sinfulness of any life within time. On the other hand, if the two stories are read as historical accounts, the disharmony between man and his environment becomes one outcome of human sin. Not merely man's attitude toward God nor his treatment of his fellow men nor his association with his fellow creatures, but the entire structure of creation and the cosmos has been altered. Earthquakes and snow, toads and lice all owe

[21] Pelikan, *The shape of death*, 107–109.

[22] *WA* **42**: 97, 35 (*AE* **1**: 130).

[23] Sun etc.: *WA* **42**: 48, 7 (*AE* **1**: 64). Serpent: *WA* **1**: 58, 16 (*AE* **1**: 76); *WA* **42**: 114, 16 (*AE* **1**: 151). "Wild" animals: *WA* **42**: 315, 7 (*AE* **2**: 74). Darnel etc.: *WA* **42**: 152, 36 (*AE* **1**: 204). Toads etc.: *WA* **42**: 156, 1 (*AE* **1**: 208). Climate etc.: *WA* **42**: 99, 29 (*AE* **1**: 133).

[24] *Cf.* Pelikan, Jaroslav, Paul Tillich und die dogmatische Tradition, *in* Heinz Brunotte *et al.*, *Gott ist am Werk*, 28–31, Hamburg, 1959.

their origin not to the love of God, but to His wrath. In fact, all of temporal existence as it is now constituted owes its origin to the wrath of God. From this it is only one step of logic to the conclusion that any life within time would have been sinful—the very conclusion that the historical exegesis of the stories of creation and fall was intended to obviate!

The irony of that logic is matched by the irony that appears in the notion of a fallen creation. At its most profound, this notion symbolizes the intuition that the harmony of life with life is the will of the Creator and that man and his fellow creatures are involved together in the whole web of existence. Only when it is almost too late do men begin to draw the moral implications of this intuition for their use of the earth and of its blessings. In the history of literature and of folklore the notion of a fallen creation has often led to the "pathetic fallacy" of ascribing to other creatures the emotions characteristic of man. Biological romanticism past and present is particularly given to the pathetic fallacy. But this is surely preferable to the callousness with which unthinking men view and treat the created world around them. Morally, emotionally, and even intellectually, romanticism is better than exploitation. Luther's historical exegesis of Genesis, with its vivid if slightly bizarre colors, could be an effective antidote to exploitation. But this same exegetical method led him to take literally the command of Genesis: "And have dominion." The dominion apparently applied even to the curse that followed sin. "Inasmuch as [the earth] sustained the sinning human being, it is also compelled, like a tool, to bear the curse. . . . The Holy Spirit . . . distinguishes between the earth and Adam: the curse He turns aside to the earth, but the person He preserves." Believing as he did that Adam was a vegetarian, Luther suggested that in the state of perfection the human race "would have made use of the creatures only for the admiration of God and for a holy joy which is unknown to us in this corrupt state of nature." [25] Even in these suggestions one can discern an anthropocentric interpretation of man's relation with other creatures. Yet it was the claim of the Reformers that they had eliminated the anthropocentric interpretation of man's relation with God that characterized medieval theology and sacramentalism.

Quite unexpectedly, however, the historical exegesis of the Genesis accounts by the Reformers did open the way for a reconsideration of all these conclusions. If one is to make sense of these accounts as an accurate record of what the universe is like and of how it came to be, one must turn to other sources to supplement the scanty information in the Bible. Despite his well-publicized remark about "that fool" Copernicus, who was trying to "turn all of astronomy upside down," Luther insisted that the study of the natural world, like the study of law or politics, had a technical autonomy and was to be permitted to carry out its research according to its own canons. Rejecting an allegory of Augustine, he declared that "the astronomers are the experts from whom it is most convenient to get what may be discussed about these subjects." "No science," he argued, "should stand in the way of another science, but each should continue to have its own mode of procedure and its own terms." [26]

The editing of Luther's *Lectures on Genesis* leads to the unforeseen discovery that he or his compilers relied heavily upon the physical writings of Aristotle for these terms and data.[27] Even in his most violent attacks on Aristotle, of course, he tried to make clear that it was the scholastic use of Aristotle that was truly pernicious, not so much Aristotle himself. Hence it was not inconsistent when he consulted Aristotle as a scientific expert. For that matter, Aristotle has performed this service for several theological traditions, Roman Catholic and Protestant. They have characteristically tended to prefer Aristotle the rationalist to Aristotle the empiricist. When it came to terms with Aristotle, the theology of the thirteenth century took a step away from the Platonic tradition and toward empiricism—as far as empiricism had developed by that time. Ironically, when the theology of the sixteenth century came to terms with Aristotle, a new empiricism, represented by men like Francis Bacon, was just beginning to come into its own.[28] And so the Aristotle who had been a bridge between theology and

[25] *WA* 42: 214, 20 (*AE* 1: 290); *WA* 42: 54, 26 (*AE* 1: 71).

[26] *WA* 42: 31, 17 (*AE* 1: 41); *WA* 42: 36, 12 (*AE* 1: 48). See the excellent summary by John Dillenberger, *Protestant thought and natural science*, 21–49, New York, 1960.

[27] *Cf.* the indices to Volumes 1 and 2 of the American Edition of *Luther's works*, *s.v.* "Aristotle." Practically all of these references to Aristotle are identified here for the first time; previous editions, including the Weimar Edition, did not indicate them.

[28] See Pelikan, Jaroslav, *From Luther to Kierkegaard*, 49–96, Saint Louis, 1950.

science became a barrier instead. But the barrier was only temporary. Eventually, men on both sides helped to tear it down, encouraged by the realization that an adequate interpretation of man and of man's world requires more insight than any single discipline can provide.

The thinker whose anniversary we celebrate in this Commemoration recognized both the potential conflict between theology and science and their mutual dependence. We may not be content with the terms of the intellectual armistice he proposed, but with its scholarly objectives most of us would probably agree: "The school of Paracelsus and some others," he wrote,

> have pretended to find the truth of all natural philosophy in the Scriptures, scandalizing and traducing all other philosophy as heathenish and profane. But there is no such enmity between God's word and his works; neither do they give honour to the Scriptures, as they suppose, but much imbase them. For to seek heaven and earth in the word of God, whereof it is said, "Heaven and earth shall pass, but my word shall not pass," is to seek temporary things amongst eternal: and as to seek divinity in philosophy is to seek the living amongst the dead, so to seek philosophy in divinity is to seek the dead amongst the living.[29]

Because, as Bacon said, there is no "enmity between God's word and his works," the validity of the message of the Hebrew and Christian Scriptures about God and man and the relation between them cannot be dependent upon the various cosmologies that appear in the Hebrew and Christian Scriptures. And theologians who claim that creation gives them special information about the cosmos need to be reminded that the God of the biblical writers transcends all cosmologies, including the cosmologies of the biblical writers. The historic achievement of the Reformation in this area, however, was not its view of the cosmos, but its witness to the meaning of creation. The same method of interpretation that led to this witness led also to scientific absurdities; meanwhile, the uncritical application of "the scientific method" to theology had discredited itself during the past century. Surely we cannot any longer afford the separation between the two cultures descended from Bacon and from Luther. It is time to banish from both cultures what Bacon called "the idols of the theater," the prejudices of our parochial traditions,[30] and to interpret both cosmos and creation in new language and with a new daring. Only if we find this new language and show this new daring shall we prove ourselves to be the faithful descendants of Martin Luther the critical scholar and of Francis Bacon the devout believer.

[29] Bacon, Francis, *The advancement of learning*, Book II, Chapter XXV, paragraph 16.

[30] Bacon, Francis, *Novum organon*, Aphorism XXXIX.

SINGLETONS AND MULTIPLES IN SCIENTIFIC DISCOVERY: A CHAPTER IN THE SOCIOLOGY OF SCIENCE *

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I

HAVING taken all knowledge to be his province, Francis Bacon made room even for what was to become sociology. His luminous writings include a charter for the social sciences, a proposed division between their several types, a precept guiding the inclusion of problems that might otherwise be lost to the view of social scientists and finally, an early, incomplete yet instructive formulation of the hypothesis to which the greater part of this paper is devoted.

With the seeming artlessness of the true artist, Bacon sets down in his *Novum Organum* what amounts to a charter for the human sciences:

> It may also be asked in the way of doubt rather than objection, whether I speak of natural philosophy only, or whether I mean that the other sciences, logic, ethics, and politics, should also be carried on by this method. Now I certainly mean what I have said to be understood of them all; and as the common logic, which governs by the syllogism, extends not only to natural but to all sciences; so does mine also, which proceeds by induction, embrace everything. For I form a history and tables of discovery for anger, fear, shame, and the like; for matters political; and again for the mental operations of memory, composition and division [this is probably Aristotle's "affirmation and negation," as Fowler makes plain], judgment and the rest; not less than for heat and cold, or light, or vegetation and the like.[1]

Not only is Bacon prepared to encompass the human sciences in his plan but he is careful to distinguish among them. Almost as though he were among us today exploring the differences and connections between the psychological and social sciences, he describes what he calls "Human Philosophy or Humanity" as having "two parts: the one considereth man segregate or distributively; the other congregate, or in society."[2]

So much for Bacon's effort to legitimate social science at a time when its first glimmerings were evident to only a few and before its sporadic development during that century of genius. No economist, Bacon could in 1615 originate the term, if not the concept, of "balance of trade"[3] the same year in which Antoyne de Montchrétien christened "political economy" (in his *Traicté de l'Œconomie Politique*). No psychologist, he could by anticipation appreciate the efforts, in mid-century, of a Hobbes, Descartes, and Spinoza to contemplate the human passions introspectively, attending to problems of perception, sensation, imagination, and the like. No great admirer of mathematics but cognizant of the value of quantification, he could write as he did generations before the extraordinary London haberdasher John Graunt, Sir William Petty, and Gregory King could among them fashion the new political arithmetic and so initiate the serious study of demography, urban sociology, and epidemiology.

Bacon did not, of course, foresee all this. Little in his time would allow him to describe social science, in the fashion Galileo described mechanics, as "the very new science dealing with a very ancient subject." But his announced philosophy of investigation allowed for such a conception. In taking note of this, we need not try to fix a particular date on which the birth of the social sciences was authoritatively registered. After all, Bacon had referred, with approving

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[1] *Novum organum*, Book I, Aphorism CXXVII.

[2] *Advancement of learning*, *The works of Francis Bacon*, collected and edited by James Spedding, Robert L. Ellis, and Douglas D. Heath, 6: 236–237, Boston, 1863 [hereafter cited as *Works*]. The pressures of time on this occasion being what they unavoidably are, I resist the temptation to remind ourselves of what Bacon goes on to say about psychosomatics (if the anachronism is allowed), when he follows precedent in writing of "the knowledge concerning the sympathies and concordances between the mind and body, which being mixed cannot be properly assigned to the sciences of either." *Ibid.*, 154 ff.

[3] In his *Letter of advice to Sir George Villiers*.

comment, to beginnings of social science before his time, reminding his contemporaries, for example, that "we are much beholden to Machiavel and others, that write what men do, and not what they ought to do," then adding, in that stately and incomparable Elizabethan prose from which peak we have achieved a steady decline, "For it is not possible to join serpentine wisdom with the columbine innocency, except men know exactly all the conditions of the serpent; his baseness and going upon his belly, his volubility and lubricity, his envy and stinge, and the rest; that is, all forms and natures of evil: for without this, virtue lieth open and unfenced."[4] And so, in what follows, but without reference to serpentine evil or columbine good, I shall try to obey the precept of Bacon, and before him of Machiavelli, by examining some of "what men [of science] do, and not what they ought to do."

Having legitimatized the social sciences, having divided them into distinct though connected disciplines and having directed us to examine the actual and not to mistake it for the ideal, Bacon gives us counsel about the scope of inquiry, urging us to give up the "childish fastidiousness" that would have us examine only those things in nature and society that we find good or pleasant or otherwise attractive. You will recall this bit of advice, destined to be echoed or independently reaffirmed in the centuries since his day by many great men of science—by a Claude Bernard or a Pasteur, among the many:

> And for things that are mean or even filthy,—things which (as Pliny says) must be introduced with an apology—such things, no less than the most splendid and costly, must be admitted into natural history. Nor is natural history polluted thereby; for the sun enters the sewer no less than the palace, yet takes no pollution. And for myself, I am not raising a capitol or pyramid to the pride of man, but laying a foundation for a holy temple after the model of the world. That model therefore I follow. For whatever deserves to exist deserves also to be known, for knowledge is the image of existence; and things mean and splendid exist alike. Moreover as from certain putrid substances—musk, for instance, and civet—the sweetest odours are sometimes generated, so too from mean and sordid instances there sometimes emanates excellent light and information. But enough and more than enough of this; such fastidiousness being merely childish and effeminate.[5]

When we consider the particular sense in which scientific discoveries can be said to come about without being dependent upon the undoubted genius of the *particular* scientists who are properly credited with these discoveries, or when we consider here in passing, what I have considered elsewhere at some length, the sociological import of the frequent clashes over priority of discovery that have marked the history of science—when I examine these and related matters, far from belittling the men of genius who have done so much to shape the development of science, I shall only be trying to fathom their distinctive and complex role in that development. Perhaps the precept of Bacon will help us find in these matters seemingly incidental to the work of scientists, "excellent light and information."

After having provided us with an attitude proper to a commemorative occasion such as this one by urging us to take up and develop the force of what the memorialized man has said rather than merely to repeat his words; after having given us a charter for the human sciences in general and having set out a useful though in the end temporary division between the primarily psychological sciences that center on "man segregate" and the primarily social sciences that center on "man congregate"; after having urged us to examine what men do and not merely what they ought to do; and after having warned us, at our peril, not to exclude the apparently mean or trivial from the scope of investigation—after he has done all this, as though it were still not enough, Francis Bacon makes my lot here an easy as well as a pleasant one by practically providing a composite text dealing with the particular subject I wish to examine: the import of a methodical investigation of singleton and multiple discoveries in science for our understanding of how science develops.

Instructed by the ideas that have been developed after Bacon's time, we can piece together from his fragmentary but instructive observations, the prime ingredients of a theory of the social processes making for discovery and invention. I say "piece together" because these ingredients are not to be found in any one place in Bacon's writings, neatly and coherently tied up in a single bundle. In part, my reconstruction is deliberate anachronism. But in part also, it is not so much

[4] *Advancement of learning*, *Works* 6: 327.

[5] *Novum organum*, Aphorism CXX. This same theme was later taken up and amplified by that thoroughgoing Baconian, Robert Boyle, in the first essay of Part I of *Some considerations touching the usefulness of experimental naturall philosophy, propos'd in familiar discourses to a friend, by way of invitation to the study of it*, Oxford, 1663.

reading into Bacon as reading him entire to gain a sense of how he conceived scientific discoveries to come about.

To begin with, Bacon wholly rejected the notion that in the new science, discoveries would typically appear at random, dropping down from heaven through the agency of star-touched genius. Instead, he declares that, once the right path is followed, discoveries in limitless number will arise from the growing stock of knowledge: it is a process of once fitful and now steady increments in knowledge. This notion of what we should today describe as the accumulative cultural base on which science builds became one of the many Baconian ideas taken up in abundance by his sometimes overly-enthusiastic disciples at mid-century. Consider only one of the more devoted of these, John Webster, who in 1654 could pleasurably refer to "our learned Country-man the Lord Bacon" as having made it clear that "every age and generation, proceeding in the same way, and upon the same principles, may dayly go on with the work, to the building up of a well-grounded and lasting Fabrick, which indeed is the only true way for the instauration and advancement of learning and knowledge." [6]

Second, Bacon holds that the individual man of science pursuing his daily labors entirely alone would at best produce small change. As he announces in the *Novum Organum*, "the path of science is not, like that of philosophy, such that only one man can tread it at a time." Consider, he says, "what may be expected from men abounding in leisure"—it would be too much to ask Bacon to foresee the excessively busy life of so many present-day scientists—"and working in association with one another, generation after generation. . . . Men will begin to understand their own strength only when, instead of many of them doing the same things, one shall take charge of one thing and one of another." [7] This theme, too, was repeatedly picked up in the seventeenth century, not least by the first historian of the Royal Society, "fat Tom Sprat," who, happily echoing Bacon, could proclaim that "single labours" in science are not enough to advance science significantly; rather, that it requires the "joynt labours of many," even to the extreme of "*joyning* them into *Committees* (if we may use that word in a Philosophical sence, and so in some measure purge it from the ill sound, which it formerly had)." [8] And still in the Baconian vein, Bishop Sprat notes that social interaction among men of science facilitates originality of conception; or as he puts it less austerely, "In Assemblies, the *Wits* of most men are *sharper*, their *Apprehensions readier*, their *Thoughts fuller*, than in their Closets." [9]

Having formulated two prerequisites for the advancement of science—the accumulating cultural base and the concerted efforts of men of science sharpening their ideas through social interaction—Bacon returns, time and again, to a third component in the social process of discovery. He tells how his proposed methods of scientific inquiry reduce the significance of the undeniably different capacities of men. You will recall the ringing passage in the *Novum Organum* to this effect:

> . . . the course I propose for discovery of sciences is such as leaves but little to the acuteness and strength of wits, but places all wits and understandings nearly on a level. For as in the drawing of a straight line or a perfect circle, much depends on the steadiness and practice of the hand, if it be done by aim of hand only, but if with the aid or rule or compass, little or nothing; so it is exactly with my plan.[10]

Read out of its immediate context, and out of the numerous other contexts in which Bacon ex-

[6] Webster, John, *Academiarum examen, or the examination of academies . . . offered to the judgment of all those that love the proficiencie of arts and science, and the advancement of learning*, 105, London, 1654. Appropriately enough, the book is dedicated to Bacon.

[7] *Novum organum*, Bk I, Aphorism CXIII. I take here the instructed translation by Benjamin Farrington, rather than that by Spedding, Ellis, and Heath, or even that by Fowler. See Farrington, *Francis Bacon*, 112, New York, Henry Schuman, 1949.

[8] Sprat, Tho., *The history of the Royal-Society of London, for the improving of natural knowledge*, 85, London, 1667; the same point of science advancing through the "joynt force of many men" or the "united *Labors* of many" recurs throughout the *History*; e.g., 39, 91, 102, 341.

[9] *Ibid.*, 98.

[10] *Novum organum*, Aphorism LXI; also CXXII. The strong-minded Macaulay made this the butt of attack in his, to some famous, to others notorious, essay on Bacon; and the even-tempered Baconian scholar, Fowler, was moved to say, "Bacon's promise never has been and never can be fulfilled." As, of course, it cannot, if it is read out of the context of the rest of Bacon's writings, so that he can be charged with gross exaggeration. But need we forget this context, better known to Fowler than to any of the rest of us? Farrington, above all others known to me, has recognized that only a misplaced and narrowly-focused literalism can lead one to assume that Bacon left no place for the great variability in the talents of men engaged in scientific inquiry. See Farrington, *op. cit.*, 116–118.

presses the same thought, this can be easily if not perversely misconstrued. It can be taken to claim that all men of science are on the same plane of capacity. It has often been so mistaken. What is more, it has often been held to affirm that all scientists are being *reduced* to the same level by the methods of science rather than being *raised* to a lofty level of competence.

But as we know from the rest of Bacon's writings, both before and after the *Novum Organum*, he meant nothing of the kind. Repeatedly, he recognizes that men have various capacities and, in his scheme of things scientific, he provides a distinctive place for each kind. That dreamt-of research institute, Solomon's House, allows for all grades of ability and varieties of skills in a complex division of scientific labor. The institute includes Merchants of Light, who keep up with the work going on in foreign countries (in the language of today, reporters of scientific intelligence); Mystery-men who gather up the earlier experiments in science and the mechanical arts (in today's terms, the men who arrange for retrieval of scientific information); Pioneers or Miners who "try new experiments, such as themselves think good" (the skilled and creative experimentalists); Compilers, or the lesser theorists, who examine the accumulated materials to draw inferences from them; Dowry-men or Benefactors who seek to apply this knowledge (men engaged in what we now call "research and development"); the Lamps, who "after divers meetings and consults of" the whole number, undertake to "direct new experiments, of a higher light, more penetrating into nature than the former" (the experimentalist directing a series of cumulative experiments); Inoculators, the technicians who "execute the experiments so directed, and report them"; and finally, his Interpreters of Nature, who "raise the former discoveries by experiments into greater observations, axioms, and aphorisms"—the pure theorist. Solomon's House makes room also for the advanced students, the "novices and apprentices" in order that the "succession of the former employed men not fail."[11]

Evidently, then, Bacon does not put all men of science on a single plane, nor does he foolishly regard them as altogether interchangeable. Rather, he emphasizes his belief that the methodical procedures of science make for greater reliability in the work of science. Once a scientific problem has been defined, profound individual differences among scientists will affect the likelihood of reaching a solution, but the scale of differences in outcome is reduced by the established procedures of scientific work. Only in this sense and to this degree, does the new science, in the Baconian image, place "all wits and understanding nearly on a level."

To the three components of his implicit social theory or discovery—the incremental accumulation of knowledge, the sustained social interaction between men of science and the methodical use of procedures of inquiry—Bacon adds a fourth and even more famous one. All innovations, social or scientific, "are the births of time."[12] "Time is the greatest innovator." He employs the same instructive metaphor to describe both his own work and that of others, as when he accounts his own part in advancing knowledge. "a birth of time rather than of wit."[13] Once the needed antecedent conditions obtain, discoveries are offshoots of their time, rather than turning up altogether at random.

To say that discoveries occur when their time has come is to say that they occur only under identifiable requisite conditions. But, of course, these conditions do not always obtain. In the past, says Bacon, inventions and discoveries have made their appearance sporadically, almost accidentally. This is so because there did not then exist the conditions of cumulative knowledge, the association of men of science and the methodical, composite use of empirical and reasoned inquiry. With the new science, all this will change. There are secrets of nature,

> . . . lying entirely out of the beat of the imagination, which have not yet been found out. They too no doubt will some time or other, in the course and revolution of many ages, come to light of themselves just as the others did; only by the method of which we are now treating, they can be speedily and suddenly and simultaneously presented and anticipated.[14]

With the increment in this passage, Bacon almost but not quite achieves a sociological conception of the development of science.

To round this out, he need only add the further component that if discoveries are "a birth of time," they will be effected by more than one discoverer. Never saying this in so many words, Bacon nevertheless intimates it—and more than

[11] *Cf.* Solomon's House.

[12] *Essays*, in *Works* **12**: 160.

[13] Aphorism CXXII, in *Works* **8**: 155.

[14] *Ibid.*, Bk. I, Aphorism CIX, in *Works* **8**: 142.

once. By paraphrasing his language, I anachronize his idea, yet without doing violence to it. What he all but says is that multiple independent discoveries do occur but not nearly so often as people suppose. The erroneous supposal is made both by those who mistakenly identify their own ideas as ancient ones and by others who claim to find in the actually new what is ostensibly old. This is how Bacon puts it:

> That of those that have entered into search, some having fallen upon some conceits [i.e. notions] which they after consider to be the same which they have found in former authors, *have suddenly taken a persuasion that a man shall but with much labour incur and light upon the same inventions which he might with ease receive from others;* and that it is but a vanity and self-pleasing of the wit to go about again, as one that would rather have a flower of his own gathering, than much better gathered to his hand. That the *same humour of sloth and diffidence suggesteth that a man shall but revive some ancient opinion, which was long ago propounded, examined, and rejected.* And that *it is easy to err in conceit* [the view] *that a man's observation or notion is the same with a former opinion,* both because new conceits [notions] must of necessity be uttered in old words, and because upon true and erroneous ground men may meet in consequence or conclusion, as several lines or circles that cut in some one point.[15]

The vice of what we may call "adumbrationism"—the denigrating of new ideas by pretending to find them old—must not be permitted to blind us to the fact that rediscovery does sometimes occur. It does not follow, however, that all newly emerging knowledge is nothing but rediscovery. Plato was mistaken in saying "that all knowledge is but remembrance."[16] In part, the error comes from the recurrent practice, particularly in "intellectual matters," of first finding the new idea strange, and then finding it exceedingly familiar.[17] In another part, the error comes from the selective perceptions of the reader. "For almost all scholars have this—when anything is presented to them, they will find in it that which they know, not learn from it that which they know not."[18] Yet apart from this common error of mistaking the new for the old in science, the fact remains that "men may meet in consequence or conclusion" despite their initial divergence of ideas. In effect, both adumbrationism and the full denial of rediscovery are faulty doctrines; the truth is, in this reconstructed judgment of Bacon, that rediscovery occurs but not as often as the adumbrationists suppose.

Now I am not saying, of course, that Bacon formulated a coherent sociological theory of the composite elements making for discovery in science. That would be adumbrationism with a vengeance. I recognize that I have pieced together his intimations of such a theory from observations scattered through the works he wrote over a span of two decades. But with the advantage of historical hindsight, and of the ideas that were formulated later, we can identify the *ingredients* of such a theory in Bacon. He himself did not see the connections between them. Or, if he saw them, he never recorded them in a form that has come down to us. What is of interest, rather, is that these ingredients should have appeared more than three centuries ago and that many men over a long period of time should have come upon them anew and that they should have begun to compose them into the beginnings of a sociological theory of scientific discovery.[19]

[15] *Valerius Terminus of the interpretation of nature* in *Works* **6**: 72-73. The emphases are mine.

[16] *Essays or counsels civil and moral*, Essay LVIII, Of Vicissitude of Things, in *Works* **12**: 273; *cf. Advancement of learning*, *Works* **6**: 88.

[17] *Advancement of learning*, in *Works* **6**: 130: "In intellectual matters, it is much more common; as may be seen in most of the propositions of Euclid, which till they be demonstrate, they seem strange to our assent; but being demonstrate, our mind accepteth of them by a kind of relation (as the lawyers speak) as if we had known them before."

[18] *De augmentis*, in *Works* **9**: 170.

[19] Bacon had much else to say that qualifies him as a harbinger of the sociology of science; I cannot deal with these matters here. But at least two sets of observations can be segregated here below to intimate the broad scope of his understanding. First, he notes the problem of the relations between the social structure and the character of knowledge: "Of the impediments which have been in the nature of society and the policies of state. That there is no composition of estate or society, nor order or quality of persons, which have not some point of contrariety towards true knowledge. That monarchies incline wits to profit and pleasure, and commonwealths to glory and vanity. That universities incline wits to sophistry and affectation, cloisters to fables and unprofitable subtilty, study at large to variety; and that it is hard to say, whether mixture of contemplations with an active life, or retiring wholly to contemplations, do disable and hinder the mind more." *Valerius Terminus* in *Works* **6**: 76. Thus we must acknowledge that he sees *the problem* of the relations between types of social structure and types of intellectual work, whatever we might think of his hypotheses. And second, he identifies all manner of social considerations that affect the ways in which men of science and learning ordinarily record what they have learned (with the intimation, perhaps, that this sorry variation will have to be suffi-

In all this, Bacon had taken hold of a salient truth: the course of scientific development cannot be understood as the work of man segregate. But he exaggerated when he went on to the claim, which remains extravagant even when construed as he evidently intended it, that the new method of science would "level men's wits and leave but little to individual excellence." In this gratuitous over-statement he is not alone. For in the centuries since Bacon, scores of observers have repeatedly stated the matter in much the same disjunctive terms: shall we regard the course of science and technology as a continuing process of cumulative growth, with discoveries tending to come in their due time, *or* as the work of men of genius who, with their ancillaries, bring about basic advances in science? In the ordinary way, these are put as alternatives: *either* the social theory of discovery *or* the "heroic" theory. What Bacon sensed, others glimpsed a little more fully, without questioning the assumed opposition of these theories of discovery. And so for more than three centuries, there has been an intermittent mock-battle between the advocates of the heroic theory and the theory of the social determination of discovery in science. In this conflict, truth has often been the major casualty. For want of an alternative theory, we have been condemned to repeat the false disjunction between the heroic theory centered on men of genius and the sociological theory centered on the social determination of scientific discovery.

ciently standardized if the institution of science is to advance knowledge, rather than to congeal it): ". . . as knowledges have hitherto been delivered, there is a kind of contract of error between the deliverer and the receiver; for he who delivers knowledge desires to deliver it in such form as may be best believed, and not as may be most conveniently examined; and he who receives knowledge desires present satisfaction, without waiting for due inquiry; and so rather not to doubt, than not to err; glory making the deliverer careful not to lay open his weakness, and sloth making the receiver unwilling to try his strength. But knowledge that is delivered to others as a thread to be spun on ought to be insinuated (if it were possible) in the same method wherein it was originally invented. And this indeed is possible in knowledge gained by induction; but in this same anticipated and premature knowledge (which is in use) a man cannot easily say how he came to the knowledge which he has obtained. Yet certainly it is possible for a man in a greater or less degree to revisit his own knowledge, and trace over again the footsteps both of his cognition and his consent; and by that means to transplant it into another mind just as it grew in his own." *De augmentis*, in *Works* **9**: 122-123; *cf.* also 16-18; *Valerius Terminus* in *Works* **6**: 70-71.

II

At the root of a sociological theory of the development of science is the strategic fact of the multiple and independent appearance of the same scientific discovery—what I shall, for convenience, hereafter describe as a multiple. Ever since 1922, American sociologists have properly associated the theory with William F. Ogburn and Dorothy S. Thomas who did so much to establish it in sociological thought.[20] On the basis of their compilation of some 150 cases of independent discovery and invention, they concluded that the innovations became virtually inevitable as certain kinds of knowledge accumulated in the cultural heritage and as social developments directed the attention of investigators to particular problems.

Appropriately enough, this is an hypothesis confirmed by its own history. (Almost, as we shall see, it is a Shakespearian play within a play.) For this idea of the sociological significance of multiple independent discoveries and inventions has been periodically rediscovered over a span of centuries. Today, I shall not reach back of the nineteenth century for cases. Let us begin, then, with 1828, when Macaulay, in his essay on Dryden, observes that the independent invention of the calculus by Newton and Leibniz belongs to a larger class of instances in which the same invention or discovery had been made by scientists working apart from one another. For example, Macaulay tells us that

> the doctrine of rent, now universally received by political economists, was propounded, at almost the same moment, by two writers unconnected with each other. Preceding speculators had long been blundering round about it; and it could not possibly have been missed much longer by the most heedless inquirer.

And then he concludes, in truly Macaulayan prose and with the unmistakable Macaulayan flair:

> We are inclined to think that, with respect to every great addition which has been made to the stock of human knowledge, the case has been similiar: that without Copernicus we should have been Copernicans—that without Columbus America would have been discovered—that without Locke we should have possessed a just theory of the origin of human ideas.[21]

[20] Ogburn, W. F., and D. S. Thomas, Are inventions inevitable? *Political Science Quarterly*, 83-98, March 1922; Ogburn, W. F., *Social change*, 90-122, New York, Huebsch, 1922.

[21] *Miscellaneous works of Lord Macaulay*, edited by Lady Trevelyan, **1**: 110-111, New York, Harper, 1880.

This is not the time to examine in detail the many occasions on which the fact of multiples with its implications for a theory of scientific development has been noted; on the evidence, often independently noted and set down in print. Working scientists, historians and sociologists of science, biographers, inventors, lawyers, engineers, anthropologists, Marxists and anti-Marxists, Comteans and anti-Comteans have time and again, though with varying degrees of perceptiveness, called attention both to the fact of multiples and to some of its implications. But perhaps a partial listing will bring out the diversity of occasions on which the fact and associated hypothesis of independent multiples in science and technology were themselves independently set forth:

In 1828—as I have said, there was Macaulay, notably in his essay on Dryden;

1835—Auguste Comte, in his *Positive Philosophy;*

1846, 1847 and 1848—the mathematician and logician, Augustus de Morgan;

1855—Sir David Brewster, the physicist, editor of the *Edinburgh Encyclopedia* and warmly appreciative though not always discriminating biographer of Newton, who was himself involved in several multiples in dioptrics with Malus and Fresnel;

1862–1864—when there was printed an entire cluster of observations upon multiples, growing out of the then-current controversy in England over the patent system, such that the *London Times* ran repeated leaders on the subject, remarking the common notoriety of the fact "that the progress of mechanical discovery is constantly marked by the simultaneous revelation to many minds of the same method of overcoming some practical difficulty" (13th September 1865);

1864—Samuel Smiles, that immensely popular Victorian biographer and apostle of self-help, repeatedly touched upon the fact of multiples;

1869—François Arago, the astronomer, physicist, biographer and permanent secretary of the Academy of Sciences, made much of multiples;

1869—Francis Galton who, in his *Hereditary Genius*, considered "it notorious that the same discovery is frequently made simultaneously and quite independently, by different persons" as attested by famous cases in point during the few years preceding and who returned to the same subject in 1874, in his *English Men of Science;*

1885—by the now little-known American anthropologists, Babcock and Pierce;

1894—Friedrich Engels, in his letter to Heinz Starkenburg, wrote of his partner in ideas that "while Marx discovered the materialist conception of history, Thierry, Mignet, Guizot and all the English historians up to 1850 are the proof that it was being striven for, and the discovery of the same conception by Morgan proves that the time was ripe for it and that indeed it *had* to be discovered";

1904—François Mentré, the French social philosopher and historian, whose basic paper, "La simultanéité des decouvertes," *Revue scientifique,* supplies a list of some 50 cases;

1905—Albert Venn Dicey, English jurist and political scientist in his magisterial *Lectures on the Relation between Law and Public Opinion;*

1906–1913—Pierre Duhem, the physico-chemist and one of the fathers of the modern history of science, who examines the fact and implications of multiples in every one of his major works;

1906—the distinguished German physiologist, Emil Du Bois-Reymond;

1913—the man who was to become the dean of American historians of science, George Sarton;

1917—the dean of American anthropologists, A. L. Kroeber;

1921—by Einstein; and then, as we near the formulation best known in the United States, in

1922—the fact and associated hypothesis of multiples as stated by the historian of science Abel Rey in France; by the then leading exponent of Marxist theory in Russia, Nicolai Bukharin; by the authoritative political scientist and essayist, Viscount Morley in England; and, of course, by Ogburn and Thomas in the United States.

The limits of my time and your patience have required me to confine this partial list to the nineteenth century and the early twentieth. But this self-set rule must be breached at least once. For on this occasion, we can scarcely exclude the observations on the subject made by the chief founder of both the American Philosophical Society and the University of Pennsylvania. Of

Franklin's several versions of the matter, I select one that bears his unmistakable imprint. Writing to the Abbé de la Roche, he remarks:

> I have often noted, in reading the works of M. Helvétius, that, though we were born and brought up in two countries so remote from each other, we have often hit upon the same thoughts; and it is a reflection very flattering to me that we have loved the same studies and, so far as we have known them, the same friends, and the same woman.[22]

Here, as elsewhere, Franklin takes the occurrence of multiples as a matter of course.

Just so do most of the others in the truncated list of multiple discoveries of the theory of multiple discoveries. That many, indeed most, of them came upon the idea independently is at least suggested by the form in which they present it, as something they have found worthy of note. Its independence is suggested also by the interest which each succeeding formulation of the idea excited among those readers who happened also to be reviewers of the book. The fact is that the theory was most unevenly diffused among scholars and scientists. By the middle of the nineteenth century, it had become, for some, a commonplace and often deplored truth; for others, it represented an entirely new conception of how science advances through the progressive accumulation of knowledge and immanently or socially induced foci of attention to particular problems by many scientists at about the same time.

Further evidence that the idea—which has, in a sense, been "in the air" for about three centuries—was being independently rediscovered is also inadvertently supplied by those critics who attacked it as thoroughly unsound or at least as ideologically suspicious. Down to the present day, some authors can bring themselves to describe the hypothesis as essentially Marxist and so, we are invited to suppose, as necessarily false. That Marx was a precocious boy of ten when Macaulay first set down his ideas on the subject and a high-spirited youth of eighteen or so when Comte asserted the same ideas—the same Comte destined to be the butt of Marx's ire—all this would appear unknown to those critics who describe the theory of multiples as entirely Marxist. What the early Victorian writers of leaders for the *London Times* would have said of this description of the hypothesis they put in print can unfortunately only be conjectured. In short, despite the many distinct occasions on which the theory of multiples was published, it has periodically emerged as an idea new to many observers who worked it out for themselves.

Even so, the fact of multiple discoveries in science continues to be regarded by some, including minds of a high order, as something surpassing strange and almost unexplainable. Here is that great pathologist and historian of medicine, William Henry Welch, on the subject:

> The circumstances that a long-awaited discovery or invention has been made by more than one investigator, independently and almost simultaneously, and with varying approach to completeness, is a curious and not always explicable phenomenon familiar in the history of discovery. . . .[23]

Other scholars tacitly assume that the pattern of multiples is both curious and distinctive of their own field of inquiry, if not entirely confined to it. As one example, consider the observation by the notable historian of geometry, Julian Lowell Coolidge:

> It is a curious fact in the history of mathematics that discoveries of the greatest importance were made simultaneously by different men of genius.[24]

And recently, the sociologist Talcott Parsons is recorded as having described the threefold, or possibly fivefold, discovery of "the internalization of values and culture as part of the personality" as "a very remarkable phenomenon because all of these people were independent of each other and their discovery is . . . fundamental. . . ." [25]

In part, of course, observations of this kind are merely casual remarks, not to be taken literally. But I should like now to develop the hypothesis that, far from being odd or curious or remarkable, the pattern of independent multiple discoveries in science is in principle the dominant pattern, rather than a subsidiary one. It is the singletons—discoveries made only once in the history of science—that are the residual cases, requiring special explanation. Put even more sharply, the hypothesis states that all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons.

Stated in this extreme form, the hypothesis must at first sound extravagant, not to say incorrigible, removed from any possible test of competent evidence. For if even historically estab-

[22] Smyth, Albert Henry, *The writings of Benjamin Franklin* 7: 434–435, New York, Macmillan, 1905–1907.

[23] Welch, William Henry, *Papers and addresses* 3: 229, Baltimore, Johns Hopkins Press, 1920.

[24] Coolidge, Julian Lowell, *A history of geometrical methods*, 122, Oxford, Clarendon Press, 1940.

[25] Parsons, Talcott, in *Alpha Kappa Deltan: a sociological journal* 29: 3–12, at 9–10, Winter 1959.

lished singletons are declared to be multiples-in-principle—potential multiples that happened to emerge as singletons—it would seem that this is a self-sealing hypothesis, immune to investigation. And yet, it may be that things are not really as bad as all that.

An incorrigible hypothesis is, of course, not an hypothesis at all, but only a dogma or perhaps an incantation. I suggest, however, that far from being incorrigible and therefore outrageous, this hypothesis of multiples is actually held much of the time by working scientists. The evidence for this is ready to hand and once its pertinence is seen, it can be gathered in abundance. Here, then, are ten kinds of related evidence that bears upon the hypothesis that discoveries in science are in principle multiples, with the singletons being the exceptional type requiring special explanation. (I wish it were possible to consider each of these kinds of evidence in detail, but that is clearly out of the question here.)

First is the class of discoveries long regarded as singletons that turn out to be rediscoveries of previously unpublished work. Cases of this kind abound. But here, I allude only to two notable instances: Cavendish and Gauss. Much of Cavendish's vast store of unpublished experiments and theories became progressively known only after his death in 1810, as Harcourt published some of his work in chemistry in 1839; Clerk Maxwell, his work in electricity in 1879; and Thorpe, his complete chemical and dynamical researches in 1921.[26] But in the meanwhile, many of Cavendish's unpublished discoveries were made independently by contemporary and later investigators, among them, Black, Priestley, John Robison, Charles, Dalton, Gay-Lussac, Faraday, Boscovich, Larmor, Pickering, to cite only a few. And in most cases, the rediscoveries were regarded as singletons until Cavendish's records were belatedly published. The case of Gauss, as we know, is much the same. Loath to rush into print, Gauss crowded his notebooks with mathematical inventions and other discoveries that turned up independently in work by Abel, Jacobi, Laplace, Galois, Dedekind, Franz Neumann, Grassmann, Hamilton, and others.[27] Again, presumed singletons turned out to be multiples, as once unpublished work became known. Far from being exceptions, Cavendish and Gauss are instances of a larger class.

What holds for unpublished work often holds also for work which, though published, proved relatively neglected or inaccessible, owing either to its being at odds with prevailing conceptions, or its difficulty of apprehension, or its having been printed in little-known journals, and so on. Here, again, singletons become redefined as multiples when the earlier work is belatedly identified. In this class of cases, to choose among the most familiar, we need only recall Mendel and Gibbs. The case of Mendel[28] is too well known to need review; that of Gibbs almost as familiar, since Ostwald, in his preface to the German edition of the *Studies in Thermodynamics* remarked, in effect, that "it is easier to re-discover Gibbs than to read him."[29]

These are all cases of seeming singletons which then turn out to have been multiples or rediscoveries. Other, more compelling, classes of evidence bear upon the apparently incorrigible hypothesis that singletons, rather than multiples, are the exception requiring distinctive explanation and that discoveries in science are, in principle, potential multiples. These next classes of evidence are all types of forestalled multiples, discoveries that are historically identified as singletons only because the public report of the discovery forestalled others from making it independently. These are the cases of which it can be said: There, but for the grace of swift diffusion, goes a multiple.[30]

[26] The detailed cases of rediscovery can be garnered from: Wilson, G., *The life of the Hon. Henry Cavendish*, London, 1851; Cavendish, Henry, *Scientific papers*, edited from the published papers and the Cavendish manuscripts, **1**: *The electrical researches*, by J. Clerk Maxwell, revised by Sir Joseph Larmor; **2**: *Chemical and dynamical*, by Sir Edward Thorpe and others, Cambridge, Cambridge Univ. Press, 1921; Berry, A. J., *Henry Cavendish: his life and scientific work*, London, Hutchinson, 1960.

[27] A preliminary list of such rediscoveries of Gauss' unpublished work has been compiled from the details in his voluminous letters—e.g., *Briefwechsel zwischen Gauss und Bessel*, Leipzig, Wilhelm Engelmann, 1880; *Briefwechsel zwischen Carl Friedrich Gauss und Wolfgang Bolyai*, Leipzig; Teubner, 1899—and in Dunnington, G. Waldo, *Carl Friedrich Gauss*, New York, Exposition Press, 1955. I shall return to the further implications of such repeated involvement of the same scientists in multiple discoveries later in this paper, when I propose a sociological concept of scientific genius.

[28] *Cf.* Iltis, Hugo, *Life of Mendel*, New York, W. W. Norton, 1932; Zirkle, Conway, Gregor Mendel and his precursors, *Isis* **42**: 97–104, June 1951.

[29] This is the entirely apt paraphrase by Muriel Rukeyser, *Willard Gibbs*, 4, 314, New York, Doubleday Doran, 1942.

[30] It is only appropriate that the original saying—"There, but for the grace of God, . . ."—should itself be, with minor variations, a repeatedly reinvented expression.

Second, then, and in every one of the sciences, including the social sciences, there are reports in print stating that a scientist has discontinued an inquiry, well along toward completion, because a new publication has anticipated both his hypothesis and the design of inquiry into the hypothesis. The frequency of such instances cannot be firmly estimated, of course, but I can report having located many.

Third, and closely akin to the foregoing type, are the cases in which the scientist, though he is forestalled, goes ahead to report his original, albeit anticipated, work. We can all call to mind those countless footnotes in the literature of science that announce with chagrin: "Since completing this experiment, I find that Woodworth (or Bell or Minot, as the case may be) had arrived at this conclusion last year and that Jones did so fully sixty years ago." No doubt many of us here today have experienced one or more of these episodes in which we find that our best and, strictly speaking, our most original inquiries have been anticipated. On this assumption, I single out only one case in point:

> The experience of Lord Kelvin as an undergraduate of 18 when he was still the untitled William Thomson, who sent his first paper on mathematics to the Cambridge *Journal* only to find that he "had been anticipated by M. Chasles, the eminent French geometrician in two points . . . [and] when the paper appeared some months later, prefixed a reference to M. Chasles' memoirs, and to another similar memoir by M. Sturm. Still later, Thomson discovered that the same theorems had been also stated and proved by Gauss; and, after all this, he found that these theorems had been discovered and fully published more than ten years previously by Green, whose scarce work he never saw till 1845."[31]

Far from being rare, these voyages of subsequent and repeated discovery by a scientist of an entire array of multiples are frequent enough to be routine.

Fourth, these publicly recorded instances of forestalled multiples do not, of course, begin to exhaust the presumably great, perhaps vast, number of unrecorded instances. Many scientists cannot bring themselves to report in print that they were forestalled. These cases are ordinarily known only to a limited circle, closely familiar with the work of the forestalled scientists. Interview studies of communication among scientists have begun to identify the frequency of such ordinarily unknown forestalling of multiples. Systematic field studies of this kind have turned up large proportions of what is often described as "unnecessary duplication" in research resulting from imperfections in the channels of communication between contemporary scientists. One such study[32] of American and Canadian mathematicians, for example, found 31 per cent of the more productive mathematicians reporting that delayed publication of the work of others had resulted in such "needless duplication," that is, in multiples.

Fifth, we find seeming singletons repeatedly turning out to be multiples, as friends, enemies, co-workers, teachers, students, and casual scientific acquaintances have reluctantly or avidly performed the service of a candid friend by acquainting an elated scientist with the fact that his original finding or idea is not the singleton he had every reason to suppose it to be, but rather a doubleton or larger multiple, with the result that this latest independent version of the discovery never found its way into print. So, the young W. R. Hamilton hits upon and develops an idea in optics and as he plaintively describes the episode:

> A fortnight ago I believed that no writer had ever treated of Optics on a similar plan. But within that period, my tutor, the Reverend Mr. Boyton, has shown me in the College Library a beautiful memoir of Malus on the subject. . . . With respect to those results which are common to both, it is proper to state that I had arrived at them in my own researches before I was aware of his.[33]

What his tutor did for Hamilton, others have done for innumerable scientists through the years. The diaries, letters and memoirs of scientists are crowded with cases of this pattern (and with accounts of how they variously responded to these carriers of bad news).

[31] Thompson, Silvanus P., *The life of William Thomson, Baron Kelvin of Largs* 1: 44–45, 113, London, Macmillan, 1910.

[32] *Cf.* Menzel, Herbert, *Review of studies in the flow of information among scientists,* Columbia University Bureau of Applied Social Research, a report prepared for the National Science Foundation 1: 21; 2: 48, January, 1960. Much other apposite information summarized in the Menzel monograph cannot be crowded into this paper. It should be added, however, that these data were uncovered in studies that were not focused on the matter of multiple and singleton discoveries; judging from the personal reports of previously undisclosed multiples that spontaneously came my way after I had published another paper on this general subject, I should judge that these occur on a scale so large that it has scarcely begun to be appreciated.

[33] Graves, Robert Perceval, *Life of Sir William Rowen Hamilton* 1: 177, Dublin, Hodges, Figgis, 1882.

Sixth, the pattern of forestalled multiples emerges as part of the oral tradition rather than the written one in still another form: as part of lectures. Here again, one instance must stand for many. Consider only the famous lectures of Kelvin at the Johns Hopkins where, it is recorded, he enjoyed "the surprise of finding [from members of his audience] that some of the things he was newly discovering for himself had already been discovered and published by others." [34]

A *seventh* type of pattern, tending to convert potential multiples into singletons, so far as the formal historical record goes, occurs when scientists have been diverted from a clearly developed program of investigation which, from all indications, was pointed in the direction successfully taken up by others. It is of course conjecture that the discoveries actually made by others would in fact have been made by the first but diverted investigator. But consider how such a scientist as Sir Ronald Ross, persuaded that his discoveries of the malarial parasite and the host mosquito were only the beginning, reports his conviction that, but for the interference with his plan by the authorities who employed him, he would have gone on to the discoveries made by others:

> The great treasure-house had been opened, but I was dragged away before I could handle the treasures. Scores of beautiful researches now lay open to me. I should have followed the "vermicule" in the mosquito's stomach—that was left to Robert Koch. I intended to mix the "germinal threads" with birds' blood—that was left to Schaudinn. I wished to complete the cycle of the human parasites—that was left to the Italians and others.[35]

Conjectural, to be sure, but with some indications that extraneous circumstances terminated a program of research that would have resulted in these discoveries becoming multiples rather than remaining adventitious singletons.

These several patterns of forestalled multiples, however, provide us with only sketchy evidence bearing on the apparently incorrigible hypothesis that multiples, both potential and actual, are the rule in scientific discovery and singletons, the exception requiring special explanation. I turn now to evidence of quite another sort, the behavior of scientists themselves and the assumptions underlying that behavior. And here, I suggest that far from being outrageous, the hypothesis is in fact commonly adopted as a working assumption by scientists themselves. I suggest that in actual practice, scientists, and perhaps especially the greatest among them, themselves assume that singleton discoveries are imminent multiples. Granted that it is a difficult and unsure task to infer beliefs from behavior; almost as difficult and unsure as to infer behavior from beliefs. But in this case, we shall see that the behavior of scientists clearly testifies to their underlying belief that discoveries in science are potential multiples.

After all, scientists have cause to know that many discoveries are made independently. They not only know it, but, act on it.[36] Since the culture of science puts a premium not only on originality but on chronological firsts in discovery, this awareness of multiples understandably activates a rush to ensure priority. Numerous expedients have been developed to ensure not being forestalled: for example, letters detailing one's new ideas or findings are dispatched to a potential rival, thus disarming him; preliminary reports are circulated; personal records of research are meticuously dated (as by Abel or Kelvin).

The race to be first in reporting a discovery testifies to the assumption that if the one scientist does not soon make the discovery, another will. This, then, provides an *eighth* kind of evidence bearing on our hypothesis. There is the good-natured race of Hahn and Boltwood, for example, to discover the "parent of radium" which Boltwood was able to find first, just as, when Hahn discovered mesothorium, Boltwood acknowledged his having been outdistanced, saying only, "I was almost there myself. . . ." [37] There is the account by Freud of how Breuer and he decided to publish their studies of hysteria sooner than originally planned as a result of Janet having begun to anticipate in print some of the salient findings.[38] There is Ramsay telegraphing Berthelot in Paris

[34] Thompson, *op. cit.* 2: 815–816. Kelvin tells of one such episode, thus: "I was thinking about this three days ago, and said to myself, 'There must be bright lines of reflexion from bodies in which we have those molecules that can produce intense absorption.' Speaking about this to Lord Rayleigh at breakfast, he informed me of this paper of Stokes's, and I looked and saw that what I had thought of was there. It was perfectly well known, but the molecule first discovered it to me."

[35] Ross, Ronald, *Memoirs, with a full account of the great malaria problem and its solution*, 313, London, John Murray, 1923.

[36] The following paragraphs are based on Merton, R. K., Priorities in scientific discovery: a chapter in the sociology of science, *Amer. Sociol. Rev.* **22**: 635–659, 1957.

[37] For a full account of this particular episode, see Eve, A. S., *Rutherford*, 164, New York, Cambridge, 1939.

[38] Freud, Sigmund, *An autobiographical study*, 36–37, London, Hogarth Press, 1948.

"at once" about his isolation of helium; writing Rayleigh to the same effect and sending a note to the Royal Society to establish priority, just as he and Travers were to announce having nosed out Dewars in the discovery of neon.[39] There is the enduring race between those two young geniuses of mathematics, Niels Abel and Karl Jacobi, to be first in each new advance of the theory of elliptic functions, with one then the other being forestalled.[40] Or, again, there is the forthright account by Norbert Wiener of the race between himself and Bouligand in mathematics, which making Wiener "aware that he must hurry," led him to get off a short note for publication in the *Comptes Rendus*, this arriving just a day after Bouligand had deposited substantially the same results in a sealed envelope at the Academy.[41] These instances are of course not atypical; Wiener is no more circumstantial and outspoken about his experience of this sort than were Wallis, Wren, Huyghens, Newton, the Bernoullis and an indefinitely large number of other scientists through the centuries whose diaries, autobiographies, letters, and notes testify to the same effect.

In all this, I exclude those cases in which scientists move to establish their priority only to ensure that their discoveries not be diffused in the community of scientists before their own creative role in them is made eminently visible or to ensure that they not be later accused of having derived their own ideas from fellow-scientists who have borrowed them or cases in which, like that of Priestley, scientists publish quickly in order to advance science rapidly by making their work available to others at once. In this class of cases pertinent to the hypothesis, I refer only to those in which the rush to establish priority is avowedly motivated by the concern not to be forestalled, for this alone is competent evidence that scientists in fact assume that their initial singletons are destined not to remain singletons for long; that, in short, a multiple is definitely in the making.

But *ninth*, not all scientists who see themselves involved in a potential multiple are prepared to be outspoken about the matter. In many cases of this sort, their scientific colleagues, or kin, are. We have only to remember the elder Bolyai, himself a mathematician of some consequence, prophetically warning his son that "no time be lost in making it [his non-Euclidean geometry] public, for two reasons:

> first, because ideas pass easily from one to another, who can anticipate its publication, and secondly, there is some truth in this, that many things have an epoch, in which they are found at the same time in several places, just as the violets appear on every side in spring. . . . Thus we ought to conquer when we are able, for the advantage is always to the first comer."[42]

Almost we hear in these words the echoed warning by other faithful colleagues of the imminent danger of being forestalled: his friend Robinson urging Oughtred to make his work on logarithms public;[43] Wallis and Halley warning Newton;[44] Halley warning Flamsteed;[45] Bache warning Joseph Henry that "no time be lost in publishing his remarks before the American Philosophical Society" now that word has come of Faraday's work on self-induction;[46] Lyell warning Darwin (Edward Blyth notwithstanding) that he must publish lest he be forestalled;[47] Bessel and Schumacher warning Gauss that he will be anticipated (as he was) on every side;[48] the elderly Legendre warning the young Karl Jacobi that the younger Niels Abel would overtake him in the race for discoveries in the theory of elliptic functions unless "you take possession of that which belongs to you by letting your book appear at the earliest possible date."[49]

Between them, Gauss and Bessel supply a beautifully ironic instance of how apt it is for scientists to assume that their original discoveries will be

[39] Travers, Morris W., *A life of Sir William Ramsay*, 133–179, London, Edward Arnold, 1956.

[40] Ore, Oystein, *Niels Henrik Abel*, 203, Minneapolis, Univ. of Minnesota Press, 1957.

[41] Wiener, Norbert, *I am a mathematician*, 92–93, New York, Doubleday, 1956.

[42] The letter is quoted in Bonola, Roberto, *Non-Euclidean geometry*, 98–99, La Salle, Illinois, Open Court Publishing Co., 2d rev. ed., 1938.

[43] *Correspondence of scientific men of the 17th century*, Stephen Peter Rigaud, ed., **1**: 7; **2**: 27, Oxford Univ. Press, 1841, 2 volumes.

[44] Weld, Charles R., *A history of the Royal Society* **1**: 408–409, London, Parker, 1848.

[45] Baily, Francis, *An account of the Revd. John Flamsteed, the first astronomer-royal, compiled from his own manuscripts*, 161, London, 1835. This case has particular point since Halley and Flamsteed were of course devoted enemies, but Halley thought it important that no English scientist be forestalled by a foreign scientist.

[46] Coulson, Thomas, *Joseph Henry: his life and work*, 109–110; 47–48, Princeton Univ. Press, 1950.

[47] Darwin, Francis, ed., *The life and letters of Charles* Darwin **1**: 426–427, 473, New York, Appleton, 1925.

[48] Dunnington, *Gauss*, 216; *Briefwechsel zwischen C. F. Gauss und H. C. Schumacher*, ed. by C. A. F. Peters, **2**: 82–83, 299–300; **3**: 69, 75; **6**: 10–11, 55, Altona, Gustav Esch, 1860.

[49] Ore, Oystein, *Niels Henrik Abel*, 203, Minneapolis, Univ. of Minnesota Press, 1957.

duplicated by others if they do not put them into print soon. For years on end, the faithful Bessel has been haranguing Gauss to publish his new discoveries on pain of being forestalled. At last, Gauss behaves as Bessel would have him behave. He publishes a treatise on dioptrics and sends a copy to Bessel who, after heroically congratulating him on the work, ruefully reports that it thoroughly anticipates Bessel's own current but still unpublished investigations.[50]

Gauss supplies us with another striking instance of the scientist's or mathematician's firm belief that a discovery or invention is not reserved to himself alone. In 1795, at the ripe age of eighteen, he works out the method of least squares. To him the method seems to flow so directly from antecedent work that he is persuaded others must already have hit upon it; he is willing to bet, for example, that Tobias Mayer must have known it.[51] In this, he was of course mistaken, as he learned later; his invention of least squares had not been anticipated. Nevertheless, he was abundantly right in principle: the invention was bound to be a multiple. As things turned out, it proved to be a quadruplet, with Legendre inventing it independently in 1805 before Gauss had got around to publishing it, and with Daniel Huber in Basel and Robert Adrain in the United States coming up with it a little later.[52]

There is a final and perhaps most decisive kind of evidence that the community of scientists does in fact assume that discoveries are potential multiples. This evidence is provided by the institutional expedients designed to protect the scientist's priority of conception. Since the seventeenth century, scientific academies and societies have established the practice of having sealed and dated manuscripts deposited with them in order to protect both priority and idea. As this was described in the early minutes of the Royal Society:

> when any fellow should have a philosophical notion or invention, not yet made out, and desire that the same sealed up in a box might be deposited with one of the secretaries, till it could be perfected, and so brought to light, this might be allowed for the better securing inventions to their authors.[53]

From at least the sixteenth century and as late as the nineteenth, it will also be remembered, discoveries were often reported in the form of anagrams—as with Galileo's "triple star" of Saturn and Hooke's law of tension—for the double purpose of establishing priority of conception and yet of not putting rivals on to one's original ideas, until they had been worked out further.[54] From the time of Newton, scientists have printed short abstracts for the same purpose.[55] These and comparable expedients all testify that scientists, even those who manifestly subscribe to the contrary opinion, in practice assume that discoveries are potential multiples and will remain singletons only if prompt action forestalls the later independent discovery. It would appear, then, that what might first have seemed to be an incorrigible, perhaps outrageous, hypothesis about multiples in science is in fact widely assumed by scientists themselves.

A great variety of evidence—I have here set out only ten related kinds—testifies, then to the hypothesis that, once science has become institutionalized, and significant numbers of men are at work on scientific investigation, the same discoveries will be made independently more than once and that singletons can be conceived of as forestalled multiples.

III

Before turning to the last part of this paper—the part dealing with a sociological conception of the role of scientific genius in the advancement of science—I think it necessary to say something about the import for the sociology of science of the fact that the theory of multiple discoveries has for so long remained largely undeveloped. After all, it is almost forty years since Ogburn and Thomas presented their list of independent discoveries and inventions. It has been at least a century and a half since observers began taking

[50] *Briefwechsel zwischen Gauss und Bessel*, Herausgegeben auf Veranlassung der Königlichen Preussischen Akademie der Wissenschaften, 531–532, Leipzig, Wilhelm Engelmann, 1880.

[51] *Briefwechsel zwischen Gauss und Schumacher* 3: 387.

[52] Dunnington, *Gauss*, 19. Adrain, the outstanding American mathematician of his day, was involved in several multiples. See Coolidge, J. L., Robert Adrain and the beginning of American mathematics, *American Mathematical Monthly* 33: 61–76, Feb. 1926.

[53] Birch, Thomas, *The history of the Royal Society of London* 2: 320, London, A. Millar, 1756. The French Academy of Sciences made extensive use of this arrangement; among the many documents deposited under seal was Lavoisier's on combustion; *cf. Œuvres de Lavoisier. Correspondance*, Fascicule II, René Fric, ed., 388–389, Paris, Michel, 1957.

[54] *Cf.* Merton, *op. cit.*, 654.

[55] *Cf.* Birch, *op. cit.* 4: 437.

formal note of the fact of multiples—even to the extent of compiling short lists of cases in point—and began to draw out the implications of the fact. And, as we have seen, it has been at least 350 years since Bacon set down some of the principal ingredients of the theory in his luminous aphorisms. Perhaps, then, it is not too soon to begin more systematic inquiry into the matter.

That such methodical inquiry may be worth the effort is at least suggested by some preliminary findings of one such study. Of the multitude of multiples, Dr. Elinor Barber and I have undertaken to examine 264 intensively. The greatest part of these—179 of them—are doublets; 51, triplets; 17, quadruplets; 6, quintuplets; 8, sextuplets. This aggregate of multiples also includes one septuplet and two nonaries, in which most of the nine independent co-discoverers were presumably ready to entertain the hypothesis that if any one of them had not arrived at the discovery, it would probably have been made in any case.

Each of these 264 multiples has been variously classified, after a search of the monographic evidence dealing with it. It has been classified in the particular discipline in which it occurred; the historical period of the multiple; the interval of time elapsing between the repeated discoveries; the number of co-discoverers; whether or not it gave rise to a contest over priority; the nationality of the co-discoverers, distinguishing those who were fellow nationals from the rest; the ages of the co-discoverers; and so on. The information about each multiple obtained through historical inquiry has been coded and transferred to punch-cards, in this way permitting detailed statistical analysis.

This is not the occasion to report the findings in hand; my purpose here is only to suggest that the intensive study of particular cases of multiple discovery can be instructively supplemented by methodical analysis of large numbers of cases. It may be of interest, for example, that 20 per cent of the multiples under review occurred within an interval of one year; some of them on the same day or within the same week. Another 18 per cent occurred within a two-year span and, to turn to the other end of the scale, 34 per cent of them involved an interval of ten years or more. The shorter the interval between the several appearances of a multiple, the *less* often does it lead to a debate over independence or other aspects of priority: of those made within a year of each other, just about half were subject to a contest over priority; of those more than twenty years apart, four in every five were contested. Ethnocentrism notwithstanding, if the independent co-discoverers are from different nations, there is slightly *less,* rather than more, probability of a conflict over priority. And to allude to just one other preliminary finding—this one, on the whole, rather encouraging—there seems to be a secular decline in the frequency with which multiples are an occasion for priority-conflicts between scientists. Of the 36 multiples before 1700 which we have examined, 92 per cent were strenuously contested; this figure drops to 72 per cent in the eighteenth century; remains at about the same level (74 per cent) in the first half of the nineteenth century and declines notably to 59 per cent in the latter half; and reaches the low of 33 per cent in the first half of this century. It may be that scientists are becoming more fully aware that, with growing numbers of investigators at work in each special field, any particular discovery is apt to be made by others as well as by themselves.

In any case, this first inquiry has been enough to persuade us that the statistical analysis of historical data bearing on discovery is a feasible and instructive next step in the sociology of science.

IV

After this interlude, I return to the last part of the sociological theory of scientific development, dealing with the role of men of genius in that development. As I have intimated, the hypothesis of multiples has long been tied to the companion hypothesis that the great men of science, the undeniable geniuses, are altogether dispensable, for had they not lived, things would have turned out pretty much as they actually did. For generations, the debate has waxed hot and heavy on this point. Scientists, philosophers, men of letters, historians, sociologists, and psychologists have all at one time or another taken a polemical position in the debate. Emerson and Carlyle, Spencer and William James, Ostwald and de Candolle, Galton and Cooley—these are only a few among the many who have placed the social theory in opposition to the theory that provides ample space for individual men of scientific genius. That so many acute minds should have for so long regarded this as an authentic debate should not keep us from noticing that the issues have been falsely drawn, that, once the two theories are clearly

stated, there is no necessary opposition between them. Instead, it is proposed that once scientific genius is conceived of sociologically, rather than, as the practice has commonly been, psychologically, the two ideas of the environmental determination of discovery and of the distinctive role of genius in discovery can be consolidated into a single theory. Far from being incompatible, the two complement one another.

In this enlarged sociological conception, men of scientific genius are precisely those men whose work in the end would be eventually rediscovered. These rediscoveries would be made, not by a single scientist, but by an entire corps of scientists. On this view, the individual man of scientific genius is the functional equivalent of a considerable array of other scientists of varying degrees of talent. On this hypothesis, the undeniably large stature of great men of science remains acknowledged. It is not cut down to size in order to fit a Procrustean theory of the environmental determination of scientific discovery. At the same time, this enlarged conception does not abandon the sociological theory of discovery in order to provide for the indisputable, great differences between men of large talent and of small; it does *not*, in the phrase of Bacon, "place all wits and understandings nearly on a level."

This enlarged sociological conception holds that men of great scientific genius will have been repeatedly involved in multiples. First, because the genius will have made many scientific discoveries altogether; and since each of these is, on the first part of the theory, a potential multiple, some will have become actual multiples; second, this means that each man of genius will have contributed the functional equivalent to the advancement of science of what a considerable number of other men of science will have contributed in the aggregate, some of these having been caught up in the repeated multiples in which the genius was actually involved.

In a word, the greatest men of science have been involved in a multiplicity of multiples. This is true for Galileo and Newton, for Faraday and Clerk Maxwell, for Hooke, Cavendish, and Stensen, for Gauss and Laplace, for Lavoisier, Priestley, and Scheele—in short, for all those whose place in the pantheon of science is beyond dispute, however much they may differ in the measure of their genius.

Once again, I can only allude to the pertinent evidence rather than report it in full. But consider the case of Kelvin, by way of illustration. After examining some 400 of his 661 scientific communications and addresses—the rest have still to be studied—Dr. Elinor Barber and I find him testifying to at least 32 multiple discoveries in which he eventually found that his independent discoveries had also been made by others. These 32 multiples involved an aggregate of 30 other scientists, some, like Stokes, Green, Helmholtz, Cavendish, Clausius, Poincaré, Rayleigh, themselves men of undeniable genius, others, like Hankel, Pfaff, Homer Lane, Varley and Lamé being men of talent, no doubt, but still not of the highest order. The great majority of these multiples of Kelvin were doublets, but some were triplets and a few, quadruplets. For the hypothesis that each of these discoveries was destined to find expression, even if the genius of Kelvin had not obtained, there is the best of traditional proof: each was in fact made by others. Yet Kelvin's stature as a genius remains undiminished. For it required a considerable number of others to duplicate these 32 discoveries which Kelvin himself made.

Following out the logic of this kind of fact, we can set up a matrix of multiple discoveries, with the entries in the matrix indicating the particular scientists involved in each of the multiples. Some of these others are themselves men of genius, in turn often involved in still other multiples. Others in the matrix are the men of somewhat less talent who, on the average, are involved in fewer multiples. And toward the lower end of the scale of demonstrated scientific talent are the far more numerous men of science, who in the aggregate are indispensable to the advancement of science and whose one moment of prime achievement came when they found for themselves one of the many discoveries that the man of genius had made independently of them.

To continue for a moment with the specimen case of Kelvin, these 32 multiples are of course only a portion of the multiples in which he was eventually involved. For, as I have said, these 32 are only the ones which Kelvin himself found to have been made by others. Beyond these, are the discoveries by Kelvin which were only later made independently by others. Of these, we do not yet have a firm estimate. And beyond these still, are what I have described as the forestalled multiples: the discoveries of Kelvin which were not, so far as the record shows, made independently by others but which, on our hypothesis,

would have been made had it not been for the widespread circulation of Kelvin's prior findings. Yet, even on this incomplete showing, it would seem that this one man of scientific genius was, in a reasonably exact sense, functionally equivalent to a sizable number of other scientists. And still, by the same token, his individual accomplishments in science remain undiminished when we note that he was not individually indispensable for these discoveries (since they were in fact made by others). This is the sense in which an enlarged sociological theory can take acount both of the environmental determination of discovery while still providing for great variability in the intellectual stature of individual scientists.

Just a few words about another like instance, in quite another field of science. Whatever else may be said about Sigmund Freud, he is undeniably the prime creator of psychoanalysis. And still, only a first examination of about a hundred of his publications finds him reporting that he was involved in an aggregate of more than thirty multiples, discoveries which he made all unknowing that they had been made by others. Once again, the pattern is much like that we found for Kelvin. Some of Freud's subsequently discovered anticipators were themselves minds of acknowledged highest order: Schiller, von Hartmann, Schopenhauer, Fechner. But many of the rest of his independent co-discoverers or anticipators are scarcely apt to be known to most of us as distinguished for the highest quality of scientific achievement; men such as Watkins Lloyd, Kutschin, E. Hacker, Grasset, Neufeld, and so on and on. It required a Freud to achieve individually what a large number of others achieved severally; it required a Freud to focus the attention of many on ideas which might otherwise not have come to their notice; in these and kindred aspects lay his genius. But that he was not individually indispensable to the intellectual developments for which he, more than any other, was historically responsible is indicated by the many multiples in which he was in fact engaged and the many others which, presumably, he forestalled by his individually incomparable genius.

What has been found to hold for Kelvin and Freud is being found to hold for all other men of undoubted scientific genius now being examined in the light of the theory. They are all scientists of multiple multiples; their undeniable stature rests in doing individually what must otherwise be done and, as we have reason to infer, at a much slower pace, by a substantial number of other scientists, themselves of varying degrees of demonstrated talent. The sociological theory of scientific discovery has no need, therefore, to retain the false disjunction between the cumulative development of science and the distinctive role of the scientific genius.

V

There is perhaps time for a few needed and self-imposed caveats. For I cannot escape the uneasy sense that this short, though you will grant me, not entirely succinct, summary of masses of data on scientific discovery must lend itself to misunderstanding. This is so, if only because so much has unavoidably been left unsaid. As a preventive to such misunderstanding, may I conclude by listing some *seeming* implications which are anything but implicit in what I have managed to report?

First, in presenting this modified version of a three-century-old conception of the course of scientific discovery, I do not imply that all discoveries are inevitable in the sense that, come what may, they will be made, at the time and the place, if not by the individual(s) who in fact made them. Quite the contrary: there are of course cases of scientific discoveries which could have been made generations, even centuries, before they were actually made, in the sense that the principal ingredients of these discoveries were long present in the culture. This recurrent fact of long-delayed discovery raises distinctive problems for the theory advanced here, but these are not unsolvable problems.

Second, and perhaps contrary to the impression I have given, the theory rejects the pointless practice of what I have called "adumbrationism," that is, the practice of claiming to find dim anticipations of current scientific discoveries in older, and preferably ancient, work by the expedient of excessively liberal interpretations of what is being said now and of what was said then. The theory is not a twentieth-century version of the seventeenth- and eighteenth-century quarrel between the ancients and the moderns.

Third, the theory is not another version of Ecclesiastes, holding that "there is no new thing under the sun." The theory provides for the growth, differentiation, and development of science just as it allows for the fact that new increments in science are in principle or in fact repeated increments. It allows also for occasional

mutations in scientific theory which are significantly new even though they are introduced by more than one scientist.

Fourth, the theory does not hold that to be truly independent, multiples must be chronologically simultaneous. This is only the limiting case. Even discoveries far removed from one another in calendrical time may be instructively construed as "simultaneous" or nearly so in social and cultural time, depending upon the accumulated state of knowledge in the several cultures and the structures of the several societies in which they appear.

Fifth, the theory allows for differences in the probability of actual, rather than potential multiples according to the character of the particular discovery. Discoveries in science are of course not all of a piece. Some flow directly from antecedent knowledge in the sense that they are widely visible implications of what has gone just before. Other discoveries involve more of a leap from antecedent knowledge, and these are perhaps less apt to be actual multiples. But it is suggested that, in the end, these too manifest the same processes of scientific development as the others.

Sixth, and above all, the theory rejects the false disjunction between the social determination of scientific discovery and the role of the genius or great man in science. By conceiving scientific genius sociologically, as the man who in his own person represents the functional equivalent of a number and variety of often lesser talents, the theory maintains that the genius plays a distinctive role in advancing science, often accelerating its rate of development and sometimes, by the excess of authority attributed to him, slowing further development.

Seventh and finally, the diverse implications of the theory are subject to methodical investigation. The basic materials for such study can be drawn from both historical evidence and from field inquiry into the experience of contemporary scientists. What Bacon obliquely noticed and many others recurrently examined can become a major focus in the contemporary sociology of science.

BACON AND MODERN PHYSICS: A CONFRONTATION *

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THE customary way to honor the memory of a great man is to single out his most meritorious contributions, the accomplishments that have made him famous, and to trace out their importance for our time. This is not the course I wish to follow, for it has always seemed to me that the errors of genius, the lack of foresight on the part of great men, are at least as interesting and often more revealing than their positive achievements. In Bacon's case, it seems curiously true that the power of modern science and its tremendous pragmatic successes spring from its allegiance with Bacon's teachings, and what we deplore in it, in its indomitability and in its apparent lack of relevance for the ethical aspirations of mankind, may owe their existence to an omission in Bacon's understanding of scientific method which subsequent generations of scientists have been slow to discover.

The comments to follow are therefore not in the nature of a eulogy but of a confrontation of Bacon with our present scene. I wish to sketch what is Baconian and what is non-Baconian in our present appraisal of the method of science; at the center of my concerns will be the problems of recent theoretical physics. Our successful understanding of them may be credited in large part to the effective use of Baconian induction; our failure to interpret their solutions in the framework of philosophy and of the totality of human experience may, it is suggested, be the result of a bedazzlement by the very successes of empirical induction. Whatever the conclusion, Bacon's figure dominates it, for he is either the beneficent progenitor of what is good in present science or else the persuasive propagandist who stamped upon it its excessively pragmatic traits.

Let me begin by stating what seems to me to be the salient characteristics of Bacon's unusual mind and of his philosophy of science. As to the former, Bacon says of himself: "I was gifted by nature with desire to seek, patience to doubt, fondness to meditate, slowness to assert, readiness to consider, carefulness to dispose and set in order." Beyond this set of attitudes, which every scientist will doubtless regard as admirable, there lies a positive view of nature, and a doctrine of how she can best be explored, which comes to the fore in Bacon's theory of forms. These forms, which are conceived somewhat in the manner of Platonic ideas, are hidden among the facts of scientific experience. Hence we must analyze experience in order to arrive at valid knowledge of the two forms with *certainty* and with *mechanical ease*. This is achieved by establishing three tables of comparative instances: (1) instances in which a given nature (form) is present; (2) instances in which a given nature is absent; (3) instances in which a given nature differs in amount. As an example, he considers the form called heat. To isolate and comprehend it, the scientist must first survey all properties which heated bodies have in common, then all properties which cold bodies have in common, and finally he must study bodies which are neither hot nor cold. The process of induction thus outlined now goes by the simpler name, the process of elimination. Underlying this view is the conviction that the number of discernible forms is small and finite, that care in investigation will isolate them, and that nature is never so complex as to resist exploration in terms of a discernible set of forms. The tables are therefore regarded by Bacon as possessing a finite number of entries. Philosophers have at times been uneasy with the simplicity of this approach, their misgivings being voiced in the old anecdote concerning the man who wished to isolate the intoxicating element of alcohol. Mixtures of gin and water, whiskey and water, vodka and water, all make a person drunk. It is concluded, therefore, that according to the Baconian directions, one must necessarily infer that the common element of these mixtures, namely water, is the cause of intoxication. There is, of course, no power in such structures, except perhaps to show that it is not always easy to employ the method properly and that nature can perhaps be sufficiently complex to make its appli-

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cation cumbersome. The process of elimination in science certainly remains as necessary and important as Bacon conceived it to be.

The structure of knowledge is reflected in Bacon's famous edifice of the pyramid of disciplines. At the bottom of all knowledge there lies the *prima philosophia*, a body of general truths. These truths are indubitable and self-evident, and their recognition is open to all unbiased men, to men who shun the idols. Upon this basis rests the discipline called natural science, the congeries of subjects often called descriptive science. Next comes physics, the science in which observation enters into combination with reasoning and calculation. The fourth and highest story of the structure of knowledge is metaphysics, the place where reason soars above observations and provides unity for the entire framework. Two things are to be noted about this arrangement: First, that the body of general truths is held to be perennial and inflexible.[1] Second is the affirmation of the validity of metaphysics, an element in Baconian thought which his disciples have frequently ignored or minimized.

The remainder of my discourse falls into three parts. The first is to deal with those aspects of modern science which are clearly in harmony with Bacon's precepts. The second will outline recent findings which transcend his proposed structure of scientific knowledge. In the third part an attempt is made to illustrate the non-Baconian elements of modern physics by reference to a well-known example, the special theory of relativity.

I. BACONIAN ASPECTS OF MODERN SCIENCE

Whenever an important scientific discovery is made and new facts are recognized, these facts are at once marshalled and assembled in accordance with the rules laid down by our philosopher. Novel understanding is brought to bear upon concrete situations; application yields all the technological results which the non-scientist often regards as the sole purpose of the scientific enterprise. Thus the obvious effect of science is technological. It is the visible and impressive movement from discovery through engineering development, the building of small-scale models, the production of new goods and devices, advertising and sale, in short, the establishment of greater comforts of life or, perchance, of greater means of destruction. This chain of events, which is established when a sufficiently significant discovery is made, in the end enhances or at any rate modifies our standard of living, our external circumstances. It is the trend which is clear and open for all to see and which has earned the applause of people in our society today. A little later I shall also speak of another trend which is launched when a deeply challenging discovery is made, a trend much less apparent and more subtle in its progression from phase to phase through scientific discovery and philosophy into human culture. To appraise the efficacy of the technological movement, however, let us consider it a little further and review some chronological incidents which reveal the speed and hence the power which the technological inductive movement has attained.

The following sketch is not intended to be a well-documented, historical account. No more can be claimed for it than that it may convey a general idea of a phenomenon visible in our society today, a phenomenon not unrelated to the scene of this discussion.

It is difficult to define a discovery or even to say when a group of discoveries can be regarded as a single major breakthrough. Nevertheless, it is often possible to assign single dates to the emergence of new ideas, and I shall deal chiefly with instances that allow this. Likewise, the assessment of a year in which a discovery was first utilized in some form of appliance may not be unique. In some examples, however, one can determine the date of a patent.

Among the nebulous discoveries of the past is that of gunpowder. Though anonymous, there is enough evidence to indicate that the discovery was made during the twelfth century as a feat of the alchemists. It is also known that gunpowder was first used in battle during the fourteenth century. A period of about two centuries intervened between discovery and utilization.

As we proceed to modern times this period is greatly contracted. The chemical process leading to the technique of photography was discovered in 1727; Daguerre's invention was patented in 1839. The telephone, which began with a discovery of Oersted in 1820, became a useful instrument in 1876. The period of maturation had been reduced to half a century. Radio began with Maxwell's equations in 1867 and led to the development of wireless devices in 1902. Television was developed between 1922 and 1934,

[1] This statement, which at any rate reflects a common view on Bacon, may be at odds with some of his writings. For a critical account see the article by C. J. Ducasse in *Theories of scientific method*, Blake, Ducasse and Madden, Univ. of Washington Press, 1960.

radar between 1925 and 1940. Fission was discovered in 1939 and the first atomic bomb exploded in 1945. The transistor began with a fundamental discovery in 1949 and was patented in 1953. If a curve were drawn in which the time of technological development is plotted against the historical date of the initial discovery, it would be seen to decrease exponentially; and if extrapolation into the future were permitted, a discovery would require only a microsecond to be patented and used in the year 2500. The very absurdity of this conjecture highlights a trend which, if it goes unchecked, may spell disaster for the human race.

I have spoken of this trend as inductive, factual, and technological, and I have ascribed its origin, by implication at least, to our philosopher. This is certainly not wholly fair. The following, however, can safely be asserted. The technological exploitation of scientific ideas which in our time is so vastly accelerated relies on the careful sifting of facts, on a comparison of effects and of the synthesizing of qualities in a manner visualized by Bacon. Statistical analysis, which is the modern counterpart of Bacon's tables of comparative instances, plays an important role in these procedures. And a minor function is assigned to basic essences, abstract meanings, logical consistency and philosophical implications which are rather irrelevant to the Baconian method.

But it cannot be denied that scientific discovery also has obscure effects in the realm of philosophy, ethics, religion, and politics. For every discovery, acting as a fact in initiating the technological trend, also becomes the leaven of an idea. As such it clamors to be understood, and the scientist must provide some sort of pervasive theory in terms of which the discovery takes on significance and organizing power. The new theory usually has features which contradict what was previously regarded as true, and, because of this apostasy, the discovery induces a rearrangement of thought in adjacent and even more distant fields of human concern. Sooner or later the internal structural consistency of the science that was disturbed by discovery is restored, and in this process some cherished beliefs, some tenets of common sense, will have to be surrendered. We note here merely the effect of the Copernican astronomical discovery upon the philosophy and, through Bruno, upon the religion of its age; the manner in which relativity ejected the time-honored concept of absolute motion; the incredible cogency with which modern quantum mechanics has annihilated the concept of solid stuff which formerly constituted matter. Results as challenging as these always have profound effects upon philosophic speculation. They are bound to alter our theory of knowledge, our understanding of the universe and of ourselves. Changes in one branch of experience will entail changes elsewhere, and even though the sequence of changes is uncontrolled and erratic, because we seem less concerned with it than we are with the technological sequence, it nevertheless tends to embrace in the end the entire structure of philosophic, moral, and even religious thought. Scientific discovery is never philosophically neutral.

The speed with which this philosophic movement reaches its culmination can be illustrated by means of a few examples drawn from the more recent past. The discovery of the laws of mechanics, which is the glorious achievement of Galileo and Newton, represents one of the deepest insights into the workings of nature. After a period of about two centuries it gave rise to the mechanical age which we must regard as the technological adjustment of the world to this new discovery. But men also sought for a philosophical transcription of the laws of mechanics. Here maximum success was obtained by Kant some 150 years after Galileo. It still seems strange to the beginning student of philosophy that a system of thought as abstract and artificial as that proposed in Kant's *Critique of Pure Reason* should have had enough persuasive power to sweep over most of the Western World. This historical miracle can be understood only when proper account is taken of the scientific antecedents of Kant's philosophy. These antecedents are found in the physics of Galileo and Newton, a branch of science unique for its singleness of claim, its universality, the categoricity with which it proclaimed its truths. When this science says that bodies attract in accordance with an inverse square law of force, as in the principle of universal gravitation, it means to say that this happens not only here but everywhere else in the universe, that it happens not only today but at all times before and after our epoch. The discovery represents a mathematically simple scheme implying that the world behaves in accordance with elegant formulas and familiar functions, suggesting in effect that the creator's mind was governed by syllogisms and mathematical equations. It is this feature which is reflected with high appropriateness in Kant's philosophy: the regularity found in the world of sense mirrors the pure

forms of reason and the categories of the human understanding.

The point we made regarding the diffusion of scientific discovery into wider fields is also well illustrated by this example. After the *Critique of Pure Reason*, Kant wrote the *Critique of Practical Reason*, and formulated the categorical imperative. It is doubtless true that the Prussian concept of duty, which on occasion embroiled the world in wars, is an outgrowth of Kant's ethics which, as we have seen, were formulated as a response to Galilean science. The time between Galileo's discovery and Kant's publications amounts to a century or two.

It is interesting to see whether this period has been shortened in our day. Let us, therefore, review two later discoveries and their philosophical interpretations. At the beginning of the nineteenth century physicists and chemists turned their attention to the laws of heat and discovered the principles of thermodynamics. These laws, in contradistinction to those of mechanics, spoke in a less certain voice; their predictions were statistical, and whatever universality they avowed arose from the laws of large numbers. Thermodynamic laws, as we now understand them, permit of exceptions although such exceptions are extremely rare. The laws caution us, because of their probability tenor, to take rare cases seriously, to reckon with the unlikely contingencies which slip through the net of science, but may have profound effects on our lives. A proper philosophical exegesis of this attitude is given by positivism, more particularly by that branch of it called logical positivism, and even more by some forms of existentialism current today. Logical positivism and existentialism seem to have reached maximum appeal in our time; hence the conclusion that the period required for understanding science in a basic philosophical way is still a century or two.

At the beginning of our era a set of challenging discoveries was made. They became the foundation of the subject now known as quantum mechanics. During the intervening fifty years we have learned to solve the equations involved, to apply them to concrete situations and to make practically all the technological predictions which the subject can possibly entail. But do we know its larger implications? Anyone who reads the current literature on the philosophy of quantum mechanics, which is very extensive, is impressed by an amazing fact: the very founders of the theory of quantum mechanics, men like Bohr, Born, Heisenberg, DeBroglie, Einstein and Schrödinger wrote searching articles on the basic meaning of their work, emphasizing the importance of philosophical interpretation; but their views did not and do not agree. Apparently the period of maturation has not yet come to an end; the contraction of the time scale we noted in connection with the technological development has not occurred.

There are cultural conclusions, important in our struggle for existence in a hostile world, to be drawn from these observations, but I shall omit them here. The relation to our theme appears to be this: Bacon has given us no recipe for establishing basic meanings, hence his program could not be carried out in the cultural-philosophic domain. Where science was able to follow him, it succeeded admirably and at an accelerated pace; where it received no directives it floundered and staggered along.

II. NON-BACONIAN ASPECTS OF MODERN SCIENCE

It behooves us to ask: Why did science fail in this latter respect; what are the features developed since Bacon's time over which his program lost control? I shall list them under three headings and give a brief description of each, as follows:

a. There is no *prima philosophia*, no ultimate, perennial, unchanging truth, at least none to be discovered by science. This recognition became general when non-Euclidean geometries were shown to be possible, consistent and useful; it speaks through the developments in modern logic and has left its imprint on the current scientific conception of axioms. These are not, according to the present version, indubitable and undeniable verities conveyed to us by some *lumen divinum* or *naturale*; they are postulates, basic but tentative hypotheses which further insight and observation can and probably will alter. The scientist's attitude toward fundamental truth is not one of final acceptance but one of commitment, not one of knowledge but of reasonable faith.

b. Science is not a set of static discoveries, completable in time, but an unending dynamism which generates new problems as it evolves. There was a time when scientific activity was likened to the solution of a picture puzzle, where pieces are placed in proper locations until a final pattern results. The pieces are the facts to be discovered and arranged, the pattern is the final solution of a scientific problem. The faults of

this simile are obvious; they lie in the supposition that science is a two-dimensional affair which exhausts itself in the arrangements of facts. We now know that understanding cannot proceed in two dimensions, that the facts which lie in the plane of the picture puzzle have roots which reach below that plane, into a domain of reasoning, of ideas, of principles which integrate the surface facts and give to them their meaning and their importance. A fact or set of facts alone can never satisfy a scientist; his concern is with the substratum of ideas at least as much as with the surface texture of the facts. Another common fallacy is the supposition that a scientific problem can ever be solved completely, a fact which is suggested by the analogy in question. On the contrary, every solution of a scientific problem presents further challenges, further problems, indeed problems in greater number than were seen before the solution was attempted.

A better analogue to the scientific process, it seems to me, is the growth of a crystal. Here we witness, first of all, a liquid matrix, amorphous in its structure, unpredictable in the chaotic motion of its constituents. Then suddenly, as the temperature of the liquid sinks below its melting point, a solid crystal begins to form. Its growth extends in all directions, although it is difficult to predict in which direction it will proceed with greatest speed. The growth is self-corrective, for occasionally the crystal will leave faults in its wake, but as it progresses it will heal its flaws. Nor does the establishment of a crystal domain pervading the volume of the former liquid change the nature of the stuff which it informs; there is no adulteration of substance but merely the generation of form, of an organized pattern, the achievement of predictability. This seems to be what the development of science does to the raw experience in its pre-scientific form.

It is clear that this dynamic process cannot be caught in static tables of forms and of comparative instances. Hence I feel that the dynamism of modern scientific enterprise is a non-Baconian element of science.

c. Science is not mere discovery, it involves human creativity; it is not inductively extracted from the facts, but constructed in large measure by man's own ingenuity. I am not at all sure that Bacon would have denied such claims; indeed had science at his time been more fully developed, he might have been the first to emphasize this point. But there was hardly the occasion for him to do so, and his disciples at later times have concentrated attention on tables of facts more or less independent of the constructs introduced by man himself. I suppose Bacon himself would have included these human contributions under his metaphysics which forms the top of his pyramid of knowledge.

Metaphysics has fallen into disrepute in some quarters. The reminder I wish to give here is that the scientist cannot get along without it in the sense that he must use principles which are not dictated or defined by observational experience alone. Reference at this point is to such matters as logical utility, verifiability, extensibility, simplicity of concepts and elegance in their use—all concerns which are regulative in scientific experience without being required by the facts alone. Even aesthetic elements are included in this list, as I hope to show in the following section.

To put things simply, one might say that man is no longer a passive, careful, and impartial spectator of nature, as Bacon would picture him, but a being who as an integral and active component of the universe can alter his own experiences and alter the objective world. In the present context, I fear that this kryptic and dogmatically assertive allusion must suffice; more is said about this point in a forthcoming book called *Open Vistas* (Yale University Press, 1961).

III. AESTHETICS AND RELATIVITY

As an illustration of the use of non-factual elements in modern science, permit me to consider with you the theory of relativity. Unfortunately, its name implies a relativism, a disposition to state laws conditionally; in short, to avoid absolutes. There is some validity in these implications when they are correctly understood, but the custom among non-scientists is to regard relativity as the doctrine that all statements are relative, indeed to hold with Protagoras that "man is the measure of all things."

In actual fact the nucleus of the theory of relativity is the idea of *invariance*, the idea that the basic laws of science do *not* change under differing circumstances. Invariance finds extensive application in the field of arts, particularly in music, painting, and in decorative design. Rhythm is, strictly speaking, invariance with respect to time displacement; repetition of ornamental pattern is invariance with respect to space displacement; the beauty of a snowflake is attributable to invariance with respect to rotation, and so on. Let me explain the meaning of these remarks.

If you look at the world through a cylindrical

telescope tube of circular cross section, you see objects whose location can be specified with respect to the edges of the tube. In particular, lines are seen to make angles with the circumference, and these angles vary between 0 and 180 degrees. Now let the tube be rotated about its axis. Nothing changes as a result of this operation, the angles remain the same as before, and indeed there is no new feature discernible from which the occurrence of a rotation could possibly be inferred. Everything remains invariant or, in more technical language, the circle which forms the cross section of the tube is invariant with respect to all rotations. For this reason, the circle is often called the perfect figure of geometry.

Imagine now that the telescope tube has a square cross section and that again a rotation through an angle differing from 90 degrees or multiples thereof is performed. The world will now look different. Lines which previously made specified angles with one of the edges of the telescope tube will now make different angles, and the fact that a rotation has occurred can be clearly discerned. Thus a square is not invariant with respect to all rotations, but only with respect to rotations through angles of 90 degrees and multiples of 90 degrees. The square is a less symmetric geometric figure than the circle, and for that reason it might be regarded as less beautiful. One aspect of the idea of symmetry is tied to the operation of rotation and draws its meaning from invariance with respect to rotation and the range of this invariance.

The meaning of relativity now becomes clearer. When the circular tube was rotated about its axis the effect of this rotation could not be detected. Absolute position, in a rotational sense, of the telescope edge was meaningless. It is thus seen that the negation of absolutes, in this case absolute orientation, is implied by the existence of an invariance. In the case of the square tube different orientations of the circumference do make sense because they can be detected operationally by comparing the views of the world which are presented under different orientations. Only orientations differing by 90 degrees and multiples of 90 degrees remain equivalent.

All regular polygons exhibit invariances with respect to rotation. Snowflakes owe their appeal to their six-fold symmetry, that is to say, their patterns are invariant with respect to rotations by sixty degrees. Invariance is thus primarily an aesthetic notion, but we shall now see how effectively it is employed in modern physics as well.

The laws of nature, when written in mathematical form, allow of certain transformations: that is to say, when specific substitutions are made for the variables which occur in them, the laws do not change their forms. All laws of physics, for example, are invariant with respect to space and time displacement, this property being in essence what is often expressed by saying that the laws are causal laws.

Earlier in this century another kind of invariance became interesting and important. It was posed in the question whether or not the laws of nature change their form when they are transformed from one frame of reference to another which is moving at constant velocity with respect to the former. Such systems are called inertial systems. It appeared from novel observations that the law of light propagation should indeed be invariant against transformations from one inertial system to another, but the accepted formula of ordinary mechanics did not bear out this expectation. Einstein thus saw himself confronted with this kind of a situation:

He beheld the simplicity and the mathematical appeal of the fundamental law of light propagation. Over against it stood common sense, a reminder coming from classical mechanics according to which certain obvious changes must be made in the variables (Galilean transformations) when passage to a new frame of reference is contemplated. Einstein had to choose between two alternatives: either to reject the aesthetic appeal of simplicity inherent in the basic law and adhere to the dictates of what seemed to have been "proved"; or else to reject common sense and retain the simple elegance of the basic law. As to the "truth" in the situation, the consequences of either step could have been justified before the facts, simply in one, more elaborately in the other alternative.

Einstein decided in favor of simplicity and elegance at the expense of common sense. He thus established the theory of relativity, and everybody knows how fruitful and illuminating his choice has been. In principle, he followed an aesthetic lead, a metaphysical conjecture, and the pay-off even in pragmatic terms was enormous. This motive, which spurns classifications and tables of comparison, is something which Bacon apparently did not foresee. Yet it is increasingly evident to the student of theoretical physics that it needs to be added to his established directives in order to provide comprehension for current problems and researches.

SCIENCE AS A FORM OF SYSTEMATIC KNOWLEDGE: SOME CONTRIBUTIONS FROM MATHEMATICS *

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THE scientist assumes that Nature is orderly; such a premise is justified by experience. To cope with some segment of Nature, therefore, requires the formulation of principles that provide an explanation of the orderliness inherent within its behavior. Once such principles have been enunciated, the scientist possesses a satisfying kind of intellectual control over a particular collection of natural phenomena; this frequently leads to varieties of actual physical control. The assemblage of all such principles is known as scientific knowledge.

Unfortunately, all we know about events in Nature is the specialized and restricted information made available to our minds by our sense organs, frequently assisted now by instruments designed to extend their effectiveness. In fact, principles promulgated to provide a rationale for the behavior of some aspect of Nature are actually designed to coordinate the pertinent sense-data, and, in that sense, to explain them.

As students of Nature have become increasingly sophisticated in their development of scientific principles, it has been discovered that man possesses the truly remarkable capacity of being able to develop models that serve to coordinate specific collections of sense-data involved in the consideration of certain kinds of natural phenomena; at least this is true to a satisfactory degree of approximation within certain ranges of the data. Thus the physical scientist, for example, can frequently offer a so-called explanation of certain relationships within a segment of Nature by discussing pertinent relationships within a mechanism, or model, that appears to possess essential elements of similarity with the aspect of Nature under consideration. That is, by reference to the proper model or models, the scientist can answer appropriate questions about certain natural phenomena; he can even predict some data if he knows other pertinent data.

A simple if imperfect analogy to the use of models by scientists can be found in the fact that the builder of a house makes his necessary determinations by reference to a blueprint. The blueprint, of course, is not the house, but it possesses elements of similarity to the house with respect to certain essential relationships.

Sir Ernest Rutherford, early in the twentieth century, developed a simple model of the atom that portrays a small mass, described as the nucleus, at the center of a series of concentric circles; upon these circles he located a collection of individual entities called electrons. By means of such a symbol, including its possible variations, many elementary questions upon atomic behavior may be answered. In fact, some persons might be tempted to believe that Rutherford's Atom is really a conventionalized sketch of an atom. But one must assume that it is merely a mechanistic device—the product, obviously, of a high order of imagination—designed to facilitate the functioning of the mind. John von Neumann warned repeatedly, "identifying the real physical or biological world with models constructed to explain it is indeed dangerous."

The evolution of the particular aspect of scientific methodology described popularly as "thinking by means of models" can virtually be identified with the history of science. Models of the heavens, designed to answer questions about the motions of the stars and the members of the solar system, were a common subject for debate in the early days of man's rational approach to the study of Nature's behavior. At a later time, a mental construct called the ether, which was assigned certain properties strange to man's experience, was created as a working basis to enable the scientist to answer specific questions pertaining to space. Phlogiston, long discarded, was created to explain many phenomena associated with the conduction of heat. Similar illustrations

*Read January 24, 1961, in the Conference on the Influence of Science upon Modern Culture, Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored jointly by the American Philosophical Society and the University of Pennsylvania.

are plentiful in the history of man's scientific studies.

Until quite recently, however, the hypothetical nature of these models was a notion generally rejected by scientists. For instance, Sir Isaac Newton, in the General Scholium at the end of Book III of the *Principia*, wrote:

> Hitherto I have not been able to discover the cause of those properties of gravity from phaenomena, and I frame no hypotheses; for whatever is not deduced from the phaenomena is to be called an hypothesis; and hypotheses, whether metaphysical or physical, whether of occult causes or mechanical, have no place in experimental philosophy.

At the present time, by contrast, most scientists pursue their studies on the assumption that it is desirable and probably necessary to employ properly conceived hypothetical models as intermediate links between the thought processes of man and the inherent reality of Nature.

By general agreement, a model designed to facilitate the answering of certain questions with respect to a specific segment of Nature must be subject to continuing scrutiny. The answers it provides must withstand the check of laboratory confirmation, at least within certain ranges of the data, when such experimental confirmation is possible. Moreover, the order of simplicity attending the use of a particular model in scientific study, especially when compared with complexities encountered in the utilization of other possible models created for the same purpose, will always be a significant factor in determining whether a model will be retained within the collection of accepted scientific principles. So, the adoption of any particular model to facilitate the study of some segment of Nature is always regarded as tentative. Interestingly enough, history reveals that man has been able in many circumstances to replace commonly accepted scientific models by new ones that seemed to possess greater utility and perhaps greater validity with respect to the objectives for which they are designed.

The major purposes of this paper are, first, to employ our knowledge of the use of models in scientific analysis to re-examine the relationship between the sciences and mathematics, and, second, to draw on the results of some recent studies in mathematics to indicate, at least by implication, some of the problems inherent in any attempt to characterize rigorously the concept of a satisfactory model.

John von Neumann has written:

> The most vitally characteristic fact about mathematics is, in my opinion, its quite peculiar relationship to the natural sciences, or, more generally, to any science which interprets experience on a higher than purely descriptive level.[1]

This I believe to be true. Perhaps a glimpse of the meaning inherent in von Neumann's assertion can be obtained through the medium of the following remarks initiated conveniently by a review of a particular episode in the history of science, namely, Maxwell's study of gases.

To facilitate his analysis of the behavior of gases, James Clerk Maxwell (1831–1879) created what he regarded as an appropriate model of a gas confined to a limited space; this model was merely a collection of little balls moving at random in a box "carrying with them their momenta and their energy." The little balls, according to his concept, would bounce off the walls of the box as predicted by the laws of mechanics; they would collide as billiard balls do; and otherwise behave as one might expect. By considering such a mechanical device he was enabled to write down a collection of symbolic propositions (the so-called Maxwell's Law of Gases suitably re-enforced with the necessary explanatory material) that actually does account remarkably well for certain recognized aspects of the behavior of a gas. Certainly the work represented a major scientific achievement. Since the time of Maxwell, however, his hypothetical mechanical model has been criticized severely. For instance, the molecules of a gas are not hard and they are not perfectly elastic; moreover their interaction is not confined to the actual moment of impact. But, in spite of such apparent fallacies, the mathematical results, that is, the collection of propositions referred to above, are still part of accepted doctrine in scientific treatises on the behavior of gases. The Maxwell propositions continue to be accepted, now without any reference to the mechanism which provided the basis for their conception, simply because they work—even though, in the words of Sir James Jeans, they "ought to have been hopelessly wrong."

The subtle possibilities of language are revealed in the masterpieces of literature. In recent centuries the literary man has found a colleague in the scientist, who has developed a strong appreciation of the powers inherent in systematic language, notably the language of logic and mathe-

[1] von Neumann, John, essay in *The works of the mind*, ed. by Robert B. Heywood, 181, Univ. of Chicago Press, 1947.

matics. In fact, the mathematician and the expert in symbolic logic have contributed important information upon problems of syntax and the use of language. Motivation for such an interest on the part of the scientist is readily found in the many notable successes that have attended the use of systematic language in scientific endeavors, especially since the sixteenth century. The Maxwell propositions may be cited as a significant illustration. There were many earlier examples, however, as exemplified by the remarkable propositions upon the motion of a satellite around the sun, enunciated by Johannes Kepler (1571–1630). These three propositions employ only a few words, but they tell so much that, through their use, remarkable predictions can be made pertaining to certain kinds of events within the solar system.

The tendency in recent times toward the extensive utilization of systematic language in scientific analysis, resulting from such successes, has been attended by a corresponding de-emphasis upon the use of schematic and mechanical contrivances. Lagrange, in fact, boasted in the preface to his *Analytical Mechanics*, published in 1788, "No diagrams will be found in this work. The methods which I expound in it demand neither constructions nor geometrical or mechanical reasonings, but solely algebraic operations subjected to a uniform and regular procedure." It may well be true, of course, that many scientific scholars have employed and will continue to employ assorted devices to aid them in their preliminary considerations before translating their results into symbolic propositions.

It was an inevitable part of this trend toward symbolism that science should attempt to employ systematic language in such a manner as to conform to the demands of science in the creation of symbolic discourses that could serve as scientific models of real or ideal situations—thereby providing the scientist a collection of symbolic mechanistic devices that would be more useful for many purposes than other kinds of models. The success of these efforts, as already illustrated by the ultimate outcome of Maxwell's endeavors, has been monumental.

And as a significant and associated element in these developments, the main stream of the history of mathematics, it now becomes clear in retrospect, is really a record of man's struggle to learn how to use the potentials of systematic language in the creation of such discourses—now called mathematical discourses—as well as the close study of their meaning and consequences. That is, at least since the time of the ancient Greeks, the mathematician has been trying to learn the art of creating a mathematical discourse, which may also be designated as a complete symbolic or verbal model, of a real or ideal situation with respect to certain elements of the situation that are specified in advance. An adequate definition of the phrase, "complete verbal model of a situation," is very difficult to frame. Tentatively speaking, however, a verbal model is complete if appropriate questions involving the designated elements of the situation may be answered by reference to the symbolic propositions of the model.

The specific situations traditionally studied by mathematicians, when contrasted with those usually considered by theoretical scientists or applied mathematicians, have possessed broad applicability in the affairs of man—and perhaps have been more simple than most of those considered in science. For instance, the mathematician has given major attention to Euclidean space, the many other kinds of space, the several varieties of number, the concepts of order, of group and so on. But the distinction just made between the common activities of the mathematician and those of the theoretical scientist or applied mathematician is quite arbitrary and trivial, especially when one desires to consider the nature of verbal scientific models and their use. The approach of the mathematician to a systematic study of Euclidean space through the creation and use of a mathematical discourse is not fundamentally different from the approach of a modern theoretical scientist who may be concerned with the development and study of a verbal model of a gravitational field, although undoubtedly it is true that the mathematician has gone farther in trying to understand his procedures.

As the mathematician has indulged in aggressive studies to learn more about his "discourses," the scientist simultaneously has moved ahead rapidly in his development and utilization of scientific models. The deliberate joining of these two streams of endeavor, although it had been implicit in much that had occurred previously, provides a significant part of the reason behind our present "explosion of knowledge."

As one gives attention to the nature of a mathematical discourse, it becomes evident that, superficially, it is merely a collection of propositions which provides a dissertation upon certain enti-

ties and their relationships as found in the situation under consideration. Actually, however, much is involved that is not immediately apparent. In fact, before going further in analyzing the properties of a mathematical discourse, it is essential to give attention to some fundamental aspects of language itself.

Each systematic language, of course, is more that a mere collection of symbols, known in the vernacular as words; it must have its appropriate syntax. That is, there must be a meta-language providing rules for the construction of propositions—"assertions about things"—out of the words of the language.

It is important to note also, from a technical point of view, that a few words of each language must be designated as primitive, or undefined. This is essential if one is to avoid circularity of definition, as employed in the dictionary. The selection of primitive terms will depend upon the nature of the situations to be considered. But, even for a particular class of situations, the language-technician is permitted considerable arbitrariness in his choice of the primitive words.

The mental gymnastics that lead to the selection of the basic concepts which become the primitive words of a mathematical discourse are little understood; such intellectual activities are included within the term, "scientific intuition." Sir George Thomson has written:

> [some concepts] show vitality and take charge, as characters in fiction are said by authors to do. They are the basis for hypotheses and "laws"; they are the things that get names—mass, energy, temperature, entropy, wavelength, charge, electrons, quantum numbers, to take some examples from my own subject. Some do not survive: . . . . Others are deposed from their independence and become vassals of the more successful. . . . But the successful concepts—or the best of them—are not concerned with details any more; they penetrate deep into the heart of things.[2]

So, the scientist-scholar, when planning a scientific dissertation organized in the manner of a mathematical discourse, will designate a small collection of the words of the language as primitive. These terms, of course, receive implicit definition through the circumstances in which they are used. It is essential, in fact, to organize a collection of so-called primitive propositions employing the primitive terms in order that such terms may receive the desired implicit definition.

[2] Thomson, George, The two aspects of science, *Science* **132**: 996, October, 1960.

The words that are employed in the construction of a mathematical discourse, other than the primitive terms, are defined explicitly by reference to the appropriate primitive terms, that is, by using familiar principles of definition. Explicit definitions employ, of necessity, a collection of words and phrases that, interestingly enough, are undefined in any language, but which represent concepts that have their origin in logical considerations; the reference is to such words or phrases as "is equivalent to" and "is included within the collection designated by." The concepts which such phrases represent are regarded as "universals" in the sense that their acceptance is antecedent to all systematic discourse irrespective of the subject or the kind of language; their nature is considered in detail in modern dissertations upon logic. The use of these "universals" is not restricted, of course, to the definitions.

Words defined explicitly are introduced from a technical point of view, obviously, for the purpose of simplifying the structure of a discourse. That is, a term defined explicitly may always be replaced in a discussion by its proper equivalent in primitive terms, but the resulting achievement may be so complex as to interfere considerably with communication and with the processes of thought. The wise introduction of new terms into a language has always provided it with greater flexibility and utility.

In brief, then, a mathematical discourse is self-contained in the sense that it designates specifically its own collection of primitive terms. All other terms of the discourse are defined by means of them. The discourse also contains a collection of statements about the primitive terms which are known as its primitive propositions. All other propositions, concerned both with the primitive and the defined terms of the discourse, are deduced logically from the primitive propositions; these derived statements are frequently called secondary propositions. In fact, through utilization of the processes of deduction that typify a particular kind of logic, accepted as an adjunct to the discourse, inherent implications of a particular collection of primitive statements—that is, what really was said in the primitive propositions—may be extracted. Significant implications of a specific set of primitive propositions may be determined through logical deduction over a considerable period of time; to carry out the deductions, moreover, may demand mathematical analysts who possess a high order of ingenuity.

An element of excitement prevails in working with the primitive propositions of Euclidean geometry, for example, which seem to be a most unimpressive lot of propositions about points and lines, when deriving from them the statement, "The sum of the interior angles of any triangle is equal to two right angles." This proposition was well-hidden in the primitive sentences. Similar illustrations may be found, of course, in all mathematical discourses, such as the various algebras, point-set theoretic topology, or Riemannian geometry.

In addition to what has already been said, the collection of primitive propositions of a mathematical discourse should possess certain properties, namely, those known as consistency and completeness. Another property, independence, is held to be desirable in certain circumstances; perhaps the most important use of this latter property, however, is in technical discussions of completeness.

A collection of primitive statements is said to be consistent if contradictory statements are not implied by the set. To be consistent is a fundamental characteristic of the collection; without it the discourse is worthless: It has been said many times that the mathematician, as he works with his mathematical discourses, is concerned essentially with consistency, not with truth.

Consistency has been established in the case of some collections of primitive propositions by the use of intuitive methods, obviously a very unsatisfactory method. No other procedure appeared to be available when the eminent German mathematician, David Hilbert (1862–1943), decided in the latter part of the nineteenth century to attempt an absolute proof of the internal consistency of certain mathematical systems. He chose to work specifically with the primitive propositions of the mathematical discourse that underlies simple arithmetic, for he knew that the consistency of many other mathematical discourses could be established if he succeeded in his efforts. Hilbert's endeavors, unfortunately, led him into grave difficulties. Finally, in 1931, Kurt Gödel clarified the nature of Hilbert's troubles in one of the most remarkable papers of our generation; it was entitled "On Formally Undecidable Propositions of *Principia Mathematica* and Related Systems." Gödel proved, in essence, that Hilbert's efforts must inevitably lead to failure. The consequences of this finding for mathematics—indeed for all of science—are vast indeed. The very fact that we must still fall back upon intuition even to establish the very important property of consistency is most disturbing.

The property of completeness, as already indicated, is a very difficult notion with which to work. Moreover, there appears to be lack of unanimity upon its meaning. A rigorous definition will not be attempted in this paper. Roughly speaking, however, the property pertains to the sufficiency of a collection of primitive propositions to enable one to work intelligently with the particular real or ideal situation for which it was designed. Specifically, a collection of primitive propositions, when complete, is ample to imply either the validity or the non-validity of a permissible statement pertaining to the situation for which the given discourse is a model; a statement to be tested is permissible, as one criterion, only if it is phrased in the vocabulary that is involved in the mathematical discourse under consideration.

Hilbert, to whom reference has already been made, developed a mathematical discourse at the very start of the twentieth century that seems truly to characterize Euclidean space—at least a kind of space that is compatible with our experiences with ordinary space. Moreover, this was the first mathematical discourse to conform to modern ideas and to be accepted generally by mathematicians. Hilbert's work is based on a collection of twenty-one primitive propositions, involving only six primitive words.

It is assumed by most informed mathematicians that Hilbert's collection of twenty-one primitive statements is complete. Consequently, by means of his discourse, geometers and others undertake to answer questions about straightline configurations and points in ordinary space—providing, of course, that the questions employ the vocabulary of the discourse. Other mathematical discourses equivalent to the Hilbert discourse in the manner in which they characterize Euclidean space are now available; specifically, they are complete if Hilbert's development is complete.

An interesting episode in the history of Euclidean geometry was associated, it is clear in retrospect, with the property of completeness. Shortly after the time of Euclid, and continuing into the nineteenth century, the so-called parallel postulate, which seemed to be quite different from the other postulates, was the subject of intense debate; its actual necessity was questioned by some geometers. Ultimately, after some of the best mathematicians of history had entered the

fray, it became clear that without the parallel postulate the Euclidean set of primitive propositions is incomplete in providing an acceptable characterization of the Euclidean plane. Moreover, it was also learned that a variety of significant non-Euclidean spaces may be characterized by the simple expedient of making suitable replacements for the parallel postulate in the total set of Euclidean primitive propositions.

Unfortunately, the remarkable studies of Gödel, to which reference has been made, also involve conclusions which imply that there always exists at least one arithmetical "truth" which cannot be derived from the primitive propositions that underlie ordinary arithmetic, irrespective of efforts to rectify the situation. Thus, from the point of view of the rigid fundamentalist, any mathematical discourse that is accepted as characterizing arithmetic will always be incomplete. This will also be true of the primitive propositions of any mathematical discourse. So it appears that a truly rigorous demonstration of the existence of the property of completeness is not to be found; this conclusion must remain a critical and disturbing element in any consideration of scientific methodology.

Through the years, mathematicians have stockpiled a great assortment of mathematical discourses for the use of the practical man and the theoretician; individual discourses have been extended in many directions by "pure mathematicians" and various aspects of the discourses have been considered with meticulous care. A surveyor, as a representative practical man, can make his measurements and then display them as a set of propositions; these propositions in union with the propositions of the mathematical discourse that characterizes the Euclidean plane and the propositions of the discourse that characterizes the rational numbers[3] provide him a sufficient and adequate basis for all his determinations as a surveyor—that is, if he has made enough measurements. Of course, the surveyor must have a background of understanding that will enable him to select the appropriate discourses to join with his propositions. The physicist, in a somewhat similar manner, may take the so-called Newtonian Law of Attraction for point-masses and join with it the propositions of the mathematical discourse that characterize the rational numbers;[3] the result will have the form of a mathematical discourse and appears to be sufficient and adequate to serve as a model in providing answers to questions pertaining to the attraction of two point-masses—at least within specific ranges of error and of data. The latter example pertains to a comparatively simple physical situation, but the same general procedure is followed by the theoretical scientist in dealing with more complex situations.

Modern scientists generally believe that they receive notable advantages when they can base their studies on scientific models cast in the form of mathematical discourses. For example, deductive reasoning as rigidly employed in mathematics is the only means yet developed for isolating hidden assumptions and for following the subtle implications of various hypotheses. Moreover, the basic entities of a science are conveniently recognized as those which are represented by the primitive terms. Some investigators will always be concerned with the reorganization of the primitive base of the discourse, even to the extent of creating a new one, and other scholars will continue to make novel deductions from the body of propositions. At times the processes of deduction reveal unexpected propositions of great consequence. Such new propositions, as indicated previously, may furnish suggestions for additional experimentation.

But the mathematicizing of science—the analysis of scientific problems through the medium of mathematical discourses employed as symbolic models—lacks much of being a panacea for all the imperfections of science. In fact, the method must never be regarded as a substitute for experiment; continuing experimentation is essential to seek confirmation of a model's value. Moreover, the use of mathematical discourses in scientific analysis neither adds to nor subtracts from anything that some persons may regard as representing truth. It is actually conceivable that such use might introduce irrelevant factors into a scientific consideration. Specifically, every theoretical scientist must exercise continual caution not to read into Nature itself some elements of fantasy that may have their true origin in inherent peculiarities of a mathematical discourse being utilized rather than in the data from Nature which the model is designed to coordinate. As Pierre Teilhard de Chardin has said, "When . . . [scientists] reach the end of their analyses they cannot tell with any certainty whether the structure they have made is the essence of the matter they are

[3] In reality the numbers of measurement are irrational numbers, but in practice, rational numbers are employed.

studying, or the reflection of their own thought." [4]

Perhaps of greatest concern, however, for those who consistently employ mathematical discourses, is the fact, stated so well by P. W. Bridgman, "It is the merest truism, evident at once to unsophisticated observation, that mathematics is a human invention." In other words, man's best-known device for facilitating the study of Nature's behavior possesses the same elements of intellectual strength and weakness that belong to man himself. The human mind itself must always be accepted as a subtle factor in any analysis that employs mathematics. Moreover, ultimate success in the development of a satisfactory mathematical discourse to be used for a specific scientific purpose requires the contribution of scholars who possess well-developed intuitive judgment, a power of the mind that is little understood but which certainly is the result of synthesis of empirical experiences.

In addition, as we have noted, even attempts to provide a logical basis to the essential properties of a mathematical discourse, which as a category includes all symbolic scientific models, seem to vanish into the mystical realm of human intuition.

There is little doubt that over a considerable period of time man has made steady progress in the field of scientific methodology. The mathematicizing of science, most scholars would agree, has been a part of that progress. But my intuition tells me, as I review the evidence, that we are beginning to near the end of an historical era. A better approach to the study of Nature's behavior, even a better utilization of mathematical knowledge in scientific endeavor, requires more understanding—very much more, in fact—of the human mind and its mode of operation. In the light of the present state of human knowledge, I must agree with Bridgman upon the significance and urgency of "the problem of adequately understanding the nature of our minds and what we can do with them." [5]

[4] Teilhard de Chardin, Pierre, *The phenomenon of man*, 32, New York, Harper & Bros., 1959.

[5] Bridgman, P. W., The logic of modern physics after thirty years, *Daedalus*, 521, Summer, 1959.

SCIENCE AS A SOCIAL PHENOMENON *

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EVERY society, even the most rudimentary, depends on scientific behavior in its daily pursuits. It must, in the first place, to keep alive. Certainly no sane human being undertakes any task without endeavoring to relate means to ends, without drawing conclusions from evidences, without applying his degree of rationality to a problem; in short, without being a scientist thus far. The primitive nomad depends on the art of hunting, and every practical art is the harnessing of a bit of science within its area. The hunter makes observations and deductions from evidences, from the traces of the softer earth or in the snow, from the sounds of the forest, from the direction of the wind, from the habits of the animals. Neither the method nor the attitude nor the practice of the scientist is the monopoly of the professional scientist. The hunter, the farmer, the businessman, the artist, the criminal, and the saint, all must resort to the way of science within the ambit of their interests and the limits of their intelligence.

Consequently, as arts and crafts developed and multiplied, a considerable amount of unsifted scientific lore, the product of intelligence directed to the manipulation of natural resources, was amassed before science was thought of as a pursuit in its own right. The arts preceded and provided the basis for the sciences. The sciences did not develop as such, until men began to seek knowledge for itself, or at least in some detachment from the needs of everyday life.

In every society, including our own, the reach of scientific development is determined by a complex of social and environmental conditions. The limitations are of several kinds—limitations of the right to pursue knowledge freely, these being set by authority and social taboos; limitations to the interest in science, these being set by the level of education and the direction of education; and limitations to the opportunity to pursue science, these being set by the distribution of wealth and poverty, by class distinctions affecting the access to education, by the natural resources available to the society and the state of the industrial arts.

If we compare the achievements of the little city-states of Asia Minor and Greece, and above all of Athens, with those of contemporary and earlier civilizations that attained a literate culture, such as the Egyptian, the Assyrian, the Indian, the Chinese, and the Israelite, we find a notable difference, in which the significance of the combination of right with opportunity stands out clearly. In the fifth century B.C. the citizen elite of Athens became the most scientific-minded group the world had ever seen. The right to inquire freely, the right to follow reason wherever it might lead, the right to question everything, sacred or profane, was roundly proclaimed. It was the message of Socrates, who knew that he did not know, and held that the exercise of reason was the only way out of the abyss of ignorance. It was the theme of the Funeral Oration of Pericles, when he lauded the Athenian freedom from bigotry that permitted men to follow their own lights and to cultivate philosophy without growing soft. In this vein Thucydides wrote the first realistic history of which we have any record, while Artistotle collected and studied the constitutions of 158 city-states and organized in his prodigious series of writings the knowledge of his age about the nature of things, about animals, and about social man. The intellectual spirit of this cultural elite was beautifully expressed by Lucretius, in a famous apostrophe to one Greek thinker, who, he declared, dared to push his thoughts out of bounds and thereby trampled under foot our dreads and dark supersititions.

Thus the right of free inquiry was signally asserted and it had remarkable consequences. The learned men of Egypt might map the heavens and know the stars by name, but it is unthinkable they would have concluded, as Herodotus did, that custom is king over men, or directly questioned the myths of the folk, as Socrates did, or denounced slavery as Euripides did, or written

*Read January 24, 1961, in the Conference on the Influence of Science upon Modern Culture, Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored jointly by the American Philosophical Society and the University of Pennsylvania.

dialogues concerning the basis of justice, as Plato did, or investigated the merits of different forms of constitution, as Aristotle did. In the surrounding civilizations the mystique of dynasty, the authority of priesthood, and the hierarchy of caste negated questionings about codes and customs and the foundations of government.

In Athens too, as in the rest of Greece, there were conditions that seriously limited the range of scientific interest. Notable as were the results of the new inquiries into the nature of social and political man, and marvelous as were the Greek achievements in architecture, sculpture, poetry, drama, and philosophy, this people made but a minor contribution to the natural sciences. We merely suggest certain conditions that stood in the way. In Athens, as in other cities, the citizen elite was an unstable little enclave, living in a city habituated to civil commotions and frequently at war with neighboring cities. They were predominantly engrossed with civic and social problems. Most of the everyday work was done by slaves, freedmen, and aliens. The life of the elite separated them from the operative life of industry and agriculture. They were wholly urbanized and their insulation kept them from addressing their inquiries to nature herself and learning from the direct study of her ways. The primary discipline in objectivity was lacking and the lack weakened the whole fabric of Greek thought.

I have sketched the complex of liberties and limitations, of rights and opportunities that released and restricted the scientific spirit of one gifted people. Every society in every period possesses no doubt enough of the inquiring and questioning attitudes to assure, were it given free range and adequate resources, some advancement of the state of scientific knowledge within it, but the degree in which it does so, if it does so at all, is determined by the particular conjuncture of the various conditions we have indicated.

Among these conditions we might single out the extent of industrial development as a primary determinant of opportunity and the ideology of government as the most significant factor in determining the direction and the limits of scientific activity. Industrial development means a demand for technological innovation, and therefore calls for scientific research in many directions. It also requires that class lines are flexible so as not to constitute educational or cultural barriers. It means the development of the arts of communication, thus enlarging the number of potential scientists. Furthermore, it means there is a range of leisure for non-utilitarian pursuits and thus for the free scientific inquiry from which come the major advances of science, while it also yields at length high dividends for the world of affairs.

Political power has some impact on science under all conditions, whether the government be democratic or otherwise. Its command of the national purse affects not only the amount spent on science, pure and applied, but also, in degree, the direction in which scientific effort is concentrated. Obviously, when the government is the agent of an authoritarian creed, it assumes, particularly through its control over education, the entire direction of science. Where its dogma is theological, the development of both the natural and the social sciences is likely to be severely restrained. Where the dogma is a non-religious or anti-religious philosophy, its restrictiveness in the area of the natural sciences is quite minor, but the social sciences are cut to the prescribed pattern and history is rewritten by way of corroboration. This type of enthroned political ideology is pre-eminently a product of the twentieth century, for while it is a commonplace of history that autocratic governments proscribe socio-economic doctrines that might threaten the established order, the Soviet system of government was the first to be founded squarely on the scriptures of a socio-economic doctrine, a doctrine that claimed to have irrefragable scientific proof for the future utopia it promised.

Aside from the intervention or the influence of established authority, freely operating social forces have at all times a considerable effect on the development of science, on the range of scientific inquiry, on the modes of inquiry, on the conceptual framework within which hypotheses are devised. There are fashions of thought, a readiness to welcome some new directions and to be sceptical of others. In retrospect we can see that theories to which significant evidences pointed might well have appeared much earlier than they did had it not been for the presuppositions of the times. The Newtonian theory, for example, might have arisen earlier but for the prestige and the sanctions of an older doctrine. The Darwinian theory of evolution met a great wave of indignant rejection before it became the accepted doctrine, and possibly on the other hand its final triumph may have been so complete that even yet it in turn has not evolved as much as it should to meet biological complexi-

ties it failed to answer. Perhaps even among scientists there is some truth in Bacon's saying that "the human understanding when it has once adopted an opinion . . . draws all things else to support and agree with it."

We must reckon also with the impact on the development of science of the social pressures of a highly organized society. Even in our own country in our own day we have witnessed censorial pressures on academic freedom. There have been attacks on scientists and efforts to oust them from colleges and universities—sometimes with success—because some intrusive committee or some propagandist body disliked their opinions. During the McCarthy heyday a determined assault was directed against economists who favored the point of view of Meynard Keynes or approved the expansion of social welfare legislation. Today the danger to academic freedom comes less from the idols of the tribe than from the idols of the pressure group.

We have offered so far a summary account of the manner in which social factors have affected the direction, the limitation, and the concentration of scientific inquiry. But to appreciate how science has become increasingly an aspect of the living texture of society it is necessary to take a broader perspective.

Science in the first place has become a profession, one with a quite special role and function. It is not so long ago since science was not a profession but at most a sideline, the avocation of a certain type of inquiring mind. The great names of the earlier stages of science were either amateurs or professors who professed not science nor any branch of science proper, but philosophy or mathematics or theology. Not infrequently they were churchmen, some of them monks. We might perhaps say they were all amateurs, in the better sense of that word. For some, it was a dangerous adventure. Bacon's Italian contemporaries in the approach to science, Tommaso, Campanella, Galileo, and Giordano Bruno were persecuted as heretics and imprisoned—and the defiant Bruno was at length burned at the stake.

Under such conditions science played a remarkable role in society, out of all proportion to its scale or to its content. In challenging the presuppositions of the established cosmology, it was insidiously threatening the established social order as well. The hierarchical interdependence of church and state, the uneasy conflict for supremacy between the two, was at length in Western Europe partially resolved in the rise of absolute monarchy. The new science challenged, even when its practitioners claimed to be devout churchmen, the right of the church to pronounce on the laws of nature. Man's reason must patiently inquire of nature herself, instead of appealing to authority. Once the catalyst of reason was applied to authority at any point, it portended developments of a revolutionary character.

In that age, when science was still a sideline of the humanities, when science had no laboratories or special instruments, when scientists were men of letters or men of action who included the scientific interest in their ambit, there was no problem of communication between scientists and the educated public, the small aristocracy of those who combined some learning with a considerable amount of social and political influence. The age of scientific professionalism was still distant.

Scientific inquiry was then a more radical solvent than it is in our own day, when the products of science-based technology arouse far more public interest than do new scientific doctrines. Through the eighteenth and the earlier part of the nineteenth century science was still relatively unspecialized, and amateurs could still make distinctive contributions to it. Benjamin Franklin is a good example. Science continued its role as disturber of the established thought-ways, but with diminishing potency. The last intense shock to popular preconceptions was that delivered by the Darwinian theory in the middle of the century.

Today science has become a vast many-department organization, with all the advantages and the problems of highly specialized enterprise. It has been estimated that between eighty and ninety per cent of all the scientists the earth has witnessed are alive today. Scientists are an army spread over an enormous terrain, moving forward from a thousand disconnected bases. The research output is so vast that no scientist can read more than a tiny fraction of the contributions being made in his major field. Some leaders in each area are concerned with the broader strategy of the advance, but the vast majority are engaging in scouting the bit of landscape in front of their noses or in occupying for technological development the immediate territory they already possess.

The necessity for specialist concentration on meticulously limited projects has a tendency to segregate the scientist in any area from his fellows in other areas. Science speaks with no one voice on any problem of social concern. The generally

educated public has no conception of what the great majority of scientists are doing. Some scientific adventures are sufficiently dramatic to receive public attention, such as the "mohole," the plumbing of the ocean depths, the Van Allen radiation belts, the mapping of the other side of the moon, and broadly, the exploration of space. There is usually one field of scientific activity that takes hold of the public imagination, although there may be little understanding of the nature or the significance of its achievements. At a crucial moment the whole world learned that the splitting of the atom had become the source of incredible power for destruction and might in time be turned to notable service for mankind. Since then, fundamental physics has held the place of honor. The atomic scientist is the wonder-worker who provides governments with the horrendous secrets of new armaments and is also in demand by great corporations which are interested in the industrial applications of atomic energy.

Never has fundamental science been so much esteemed and never, perhaps, has the intrinsic significance of scientific advance been so little comprehended. Some scientists have themselves stated that the realm of the atom is so different from the macroscopic earth of tangible and visible things, so remote even from the conceptual images and abstractions that other sciences deal with, so exclusively construed in special symbols and mathematical formulas, that the new knowledge cannot be conveyed even to what we call the educated public.

No doubt it is true that the more elaborate procedures and the reasoning they embody and the background of training on which they depend are not communicable to those who lack the experience of the atomic scientist. No doubt also no one who lacks that experience can have the same quality of recognition, the same feel of the texture of things, that belongs to the specialist. To the uninitiate the mere statement of conclusions, of particular findings, remains detached, cold, unincorporate within his sketchy notions. But to some extent similar statements apply to the knowledge of every science, of every fine art, of every area that calls for *expertise*. The atomic scientist no doubt thinks in a context utterly remote from the experience of other men, in a world of infinitesimals wherein the resort to visual imagination is balked. As a total outsider from this world I would not venture to defend the position that a rough understanding of the significance of the advances in atomic science can still be communicated to a much larger public. I fall back on the words of a physicist who is himself a notable communicator, Werner Heisenberg. "Whenever we proceed from the known to the unknown," he says in his *Physics and Philosophy*,

> we may hope to understand, but we may have to learn at the same time a new meaning of the word "understanding." We know that any understanding must be based finally upon the natural language because it is only there that we can be certain to touch reality, and hence we must be sceptical of any scepticism with regard to this natural language and its essential concepts. Therefore, we may use these concepts as they have been used at all times. In this way modern physics opened the door to a wider outlook on the relation between the human mind and reality.

Scientists, some scientists, may regard their particular scientific enclave as incommunicate, having nothing to say that can be translated into the language of the people, but science itself, science in some form, in some mode of social translation, is always an essential factor in the outlook of every community. Where scientific knowledge is lacking or ignored, pseudo-science is substituted. No great social controversy can be carried on without appeal either to science, which may be rightly or wrongly apprehended, or to pseudo-science. Take the conflict between the ideologies of communism and anti-communism. The lavish citation of alleged facts is always related to some doctrine that is presumptively corroborated by the alleged facts. And the doctrine is always related to presumptions about human nature, about the course of history, about the chain of consequences of different systems of production, and looming beyond such presumptions there is on the one side a philosophy of materialism that has biological implications and on the other side a variety of beliefs in the reality of non-material or spiritual values. As Francis Bacon said, it is a tendency of mankind "through the premature hurry of the understanding to leap or fly to universals or principles of things," and "great danger may be apprehended from philosophies of this kind"—at least, we may add, when, armed with power, they clash with one another. Or take the present issue over racial segregation in our own country. There are arguments on the effects of miscegenation, on the equality or superiority of racial stocks, on the lessons to be

learned from history, on the psychological consequences of discrimination, on the economic benefits of equalized opportunity, and so forth—all topics where the final appeal is to science, and not to the emotions that so readily assume they have science on their side. Man is a most argumentative animal and the first premise of his endless arguments is attributed either to a science of which he knows little or to a God he does not know at all.

It seems obvious, therefore, that the more the findings of science, wherever at least they have any relevance to human affairs or human relationships or broadly to man's outlook on the world of nature or the cosmos itself, are effectively communicated and interpreted to the public, the more likely it is that social and political decisions will be more enlightened than otherwise they would be. Science is a condition and a partner of every art, including the art of living. Recently, an author who is both an effective scientist and a distinguished novelist, C. P. Snow, claimed that in our present civilization the arts and the sciences form two separate cultures, neither of which has concern for or understanding of the other.[1] So far as this may be true, it is an unhappy divorce, unfortunate for the society they share. Science and the humanities, to use an old term that itself may be misleading, are closely interwoven into the endless process of society. Science changes more rapidly, moves only in one direction, to more advanced science. The humanities move often in a seemingly directionless manner, but they age better; their major achievements are not outmoded or replaced by superior ones. In this and in other respects the sciences and the arts are curiously complementary. Scientific advance, when translated into the products of technology, can have a disturbing or even disruptive effect on social standards, on the equilibrium of the folkways, unless there is enough recognition of the consequences and enough preparation to assure cultural adjustment to the new conditions. The salient example is the harnessing of atomic power, which renders obsolete age-old traditions concerning war and sovereignty.

In this respect scientists serve society not only by enlightening the public on the realities of the world we live in but also by helping to guide it in the intelligent use of knowledge, as at the present time many atomic scientists are notably doing. Science abjures value judgments but the scientist has no similar privilege. He is no more free than other men from the responsibilities of a citizen.

The line of communication between science and society is weakened by the habits and conditions of modern specialization, and communication is too easily dismissed as popularization, something the respectable professional abhors. The outlook of human beings is curiously compounded of scientifically valid, scientifically neutral, and scientifically invalid beliefs, and unfortunately the third ingredient, being congenial to the passions that prompt such beliefs, is often the most vehement and overbearing of the three. In times of tension and crisis it is, therefore, likely to take complete control, a phenomenon of which we have had tragic examples in recent times.

It may be too hard a task for the scientist to instill into the public mind something of his own scientific outlook, but surely the broader conceptions of the nature of the cosmos, of man's place in it, of the limitation and yet the marvelous advances of our knowledge, of the relation of mind and body, of the evolution of *homo sapiens*, could be included in the education of every educable citizen. It is this perspective, the fruit of knowledge, rather than the gobbets of information the pupil memorizes and forgets, that, properly conveyed, can subtly change the attitudes of men and blunt the gross prejudices to which the untutored imagination is so prone. Without being too optimistic about human nature, one might wonder whether, had the relevant knowledge been available and sufficiently widespread, certain pernicious doctrines would ever have gained control. One might wonder, for example, whether the early church fathers could have entertained such ignoble ideas of what was called the "flesh," which involved among other unhappy consequences the degradation of women, had there been any sufficient knowledge of the endlessly intricate marvel of reproduction. Or, to jump to modern times, had the youth of Germany been taught the relation of racial and ethnic distinctions to the common human stock, and the manner in which the diverse peoples have contributed to the greater culture of mankind, instead of the chauvinistic interpretation of history that passed muster in so many schools, would the notorious bigot, Adolf Hitler, have been so successful in his disastrous march to power? Or perhaps again, had the Southern segregationists been taught in their schoolbooks some honest an-

[1] The two cultures and the scientific revolution, The Reith Lecture, 1959, Cambridge University Press.

thropology, instead of the curious legends most of them still believe, would they have been so unprepared to face a decision that had to come, sooner or later? Relevant knowledge does in time change attitudes and modify our conceptions of self-interest. There is that element of truth in the old Socratic notion that knowledge is virtue. The lack of knowledge is certainly the ground of error, and every society is beset by misunderstandings of many kinds, misunderstandings of human nature, misunderstandings of our place in nature, misunderstandings of our relationships with others. Knowledge is not wisdom but it is the compass of wisdom. From all of which it surely follows that the effective communication of knowledge, of the broad findings of the sciences and of the scientific point of view, is the first obligation of science to society.

SCIENCE AS TECHNOLOGY *

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THE title of this paper, Science as Technology, would not have surprised or shocked Francis Bacon, for perhaps his most original contribution to the enlargement of the province of science was his understanding of its great future role in transforming the physical conditions of life. But I am sure that the conclusions that I shall finally present—conclusions in the form of doubts, challenges, and questions—would have shocked him quite as much as it will, I fear, shock many of those who are here this morning, for his faith in science as a source of technology, and in technology itself as the final justification of science must now, after four centuries, be submitted to an historic evaluation and the pragmatic test. When Bacon's assumptions are rigorously examined, they should, I submit, lead to a modification of Bacon's original hopes and even a radical change in our own attitude toward many Baconian beliefs we have, somewhat blindly, taken to be axiomatic.

Doubtless it is natural, in celebrating Bacon's anniversary, that we should out of piety overemphasize those aspects of modern civilization that have confirmed his predictions and surpassed his none-too-cautious expectations. This is particularly true when we consider science as technology, for it is precisely in this department that his most extravagant intuitions have been realized. Three centuries before Jules Verne and H. G. Wells, to say nothing of later writers of science fiction, Bacon anticipated the multifold uses that technology would make of science.

Though Bacon was undoubtedly expressing, as a sensitive artist often does, the changing temper of his age, long before it was visible in the streets, his very predictions gave confidence in the new orientation toward the physical world as the only area in life sufficiently detached from subjective fantasies and emotional urges to serve as a common meeting ground for minds otherwise ideologically separated. Men who could not agree upon the nature of God, could come to terms by making a god out of nature, once they had hit upon a method that ruled out all experience that could not be experimentally repeated or independently verified. By following through the practical consequences of science, Bacon sought to show even those who were engaged in the most abstract calculations and experiments that they might ultimately confer greater benefits upon the race than those who were laboring to improve it by law, by morals, or by government, or who sought to change the environment solely by manual labor and art.

Now the notion that the scientific observation of air, earth, water, and fire might lead to fruitful applications in technology, must have occurred to many minds, Archimedes for one, Hero of Alexander for another, and Bacon's medieval namesake for a third, before Francis Bacon himself elaborated the idea. But Bacon helped mightily to close the gap between the spheres of science and technology, one long considered liberal but exquisitely useless, except perhaps in medicine, the other, however useful, cursed by its servile and debasing nature. Bacon held that the advancement of knowledge depended upon more than the abstract, logic-disciplined exercises of mind. He felt that science in future would rest increasingly on a collective organization, not just on the work of individuals of ability, operating under their own power; and he held, further, that instruments and apparatus were as necessary in the technology of systematic thought as they were in mining or bridge-building.

"The unassisted hand," observed Bacon, "and the understanding left to itself possess little power." This was an even more revolutionary conception than Leonardo da Vinci's aphorism: "Science is the captain, practice the soldiers"; for it implied that the captain himself had something to learn from the men in the ranks. And it was no less revolutionary, no less effective, because, from the standpoint of a mature scientific method

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it was, by overcompensation, too one-sided. Bacon's very overemphasis on the collective apparatus of science, his close concern for the operational and instrumental aspects of scientific thought, were probably needed to overcome the bias of traditional leisure-class culture, theological and humanistic, operating by choice in a social vacuum. That was a necessary contribution in his own day—as necessary as the opposite position may become in our own time.

The timeliness and significance of Bacon's contribution here should have saved him from a little of the patronizing deflation that he has been subject to in recent years. Without doubt, he was blandly indifferent to the actual procedures of successful scientists in his own time, like Gilbert and Galileo; and, further, it is no doubt true that Bacon grossly overestimated the fruitfulness of mere fact-collecting and fumbling empirical observation, though there are still areas where this kind of systematic preparatory effort yields a certain reward. By the same token, Bacon seriously underestimated, one might almost say he entirely ignored, the immense liberation that would be effected in both science and technics through the audacities of pure mathematics, dealing with possibilities and probabilities that are, until experimentally verified, outside the field of direct observation and sensory experience.

On his own terms Bacon could not and did not anticipate the sweeping transformations of the entire framework of thought effected by single minds, almost destitute of apparatus, like Newton, Clerk Maxwell, or Einstein. Even Galileo's scientific world, a world conceived solely in terms of primary qualities and measurable quantities, was almost unthinkable to Bacon. But to offset these disabilities, which plainly reduce Bacon's importance as a philosopher of science, he had a strong sense of the sociological context of science, and of the appeal that this would make to scientists, to inventors, to engineers, and to their countless human beneficiaries. He foresaw that science would become a corporate enterprise, subject to deliberate organization; and that the social goal of science, as he phrased it in The New Atlantis, would be "the enlargement of the bounds of humane empire, to the effecting of all things possible."

Curiously, what is most fresh and original in Bacon, his conception of the role of science as the spiritual arm, so to speak, of technology, is the hardest part for our contemporaries to appreciate fully today. Partly they are put off by the fact that he absurdly arrayed these new conceptions in an elaborate metaphorical court dress; but even more they are alienated, or to speak more frankly, bored, because the ideas themselves have become so engrained in our life that most of us can hardly realize that they had a specific point of origin. But if Bacon failed miserably in interpreting the methodology of science, as it was actually taking shape in his own time, he leaped ahead four centuries to the mode and milieu in which science and technics both flourish, in their peculiar fashion, today. When Benjamin Franklin founded the American Philosophical Society, he felt it necessary to stress its aim of promoting "useful knowledge": but if he had been even closer to Bacon's spirit he would have realized that usefulness is implicit in every kind of scientific knowledge, almost it would seem in proportion to its degree of abstraction and its isolation from the immediate practical concerns. The singular mission of science, as a technological agent, is to suggest uses and outlets, issuing from purely theoretic and experimental discoveries, that could not have been conceived until the scientific work itself was done.

In the past, certain branches of science, like geometry, had developed out of practical needs, like the Egyptian need for surveying anew the boundaries that had been effaced in flooded fields; and some of that interplay between practical needs and scientific investigation of course still goes on, as in the classic instance of Pasteur's researches on ferments in response to the pleas of French wine growers. But the enormous advances of science in every field have not waited for such direct stimuli, though it may very well be that they are indirect responses organically connected with the needs and purposes of our society at a hundred different points. Thus, it is quite probably not by accident that the electronics of radar location have coincided with coordinate advances in the physics and technology of high-speed flight. Increasingly, however, it is the advance of science that suggests a new technological application: indeed the technological by-products seem to multiply in direct relation to the scope and freedom of scientific research. So ready are we now to accept the inventive consequences of science that we have almost lost the safeguard of common sense or the protective device of laughter, against freaks and follies and stunts unrelated to human need, but scientifically

conceivable and technologically practicable—like the present frantic and humanly extravagant efforts to land astronauts on the moon.

Bacon's interest in the practical applications of science naturally endeared him to Macaulay and the other utilitarians of the nineteenth century, for in his *Novum Organum* Bacon boldly asserted that the "legitimate goal of the sciences is the endowment of human life with new inventions and riches." This is a more questionable goal than Bacon thought: but it is because of the accelerated fulfillment of these promises by the sciences, especially during the last half-century, that national governments and great industrial corporations have vastly augmented their financial contributions to scientific research. Bacon's merit was to make plain that there was no aspect of nature that would not lend itself to transformation and improvement through the unrestricted application of the experimental method. Necessity had always been the most reluctant mother of invention: Bacon understood that curiosity was a far more fertile parent, and that the inventions so promoted would become the mother of new necessities.

But Bacon went further: he saw that curiosity, to be fully effective, must enlist, not solitary and occasional minds, but a corps of well-organized workers, each exercising a specialized function and operating in a restricted area. By the technological organization of science as he portrayed it in the *New Atlantis*, he proposed to fabricate an engine capable of turning out useful knowledge in the same fashion that a well-organized factory would, shortly after Bacon's prediction, turn out textiles or shoes. Bacon's description of this division of labor strikes us as quaint and finicking, because of its static, ritualistic assignment of tasks; but those who would dismiss it altogether are wider of the mark than Bacon; for part of the immense quantitative output of contemporary science is surely due to its ability to make use, not only of a few great directive minds, but of a multitude of specialized piece workers, narrowly trained for their tasks, deliberately denied any individual opportunity to explore a wider field; whose part in the whole process increasingly resembles that of a factory worker on an assembly line. The corporate personality has taken over the attributes of the individual thinker; and as science comes more and more to rely for its results upon complicated and extremely expensive apparatus, like electronic computers and cyclotrons, no work along present lines can be done without close attachment to a corporate organization. The dangers that this technological advance offers to science have not yet been sufficiently canvassed; but they will perhaps nullify no small part of its rewards.

Bacon's conception of the organization of science as a technology did not altogether overlook the part played by individual creative minds: he even had a name for such seminal investigators, for he called them "Lamps," and indicated that their function was to "direct new experiments of a Higher Light, more penetrating into nature." But his peculiar contribution was to sense that, if the illuminations and insights of creative minds were to have the widest kind of application, they would need abundant collective support: state aid, corporate organization, systematic conferences and publications, liberal rewards and honors, and finally, public exhibition and celebration in museums of science and industry. It was these features of collective organization and state regimentation, not perhaps entirely unknown in pre-Christian Alexandria, that Bacon so presciently recognized, advocated, and exalted. So it was not only the Royal Society or the American Philosophical Society that Bacon anticipated: his quaint account of the future in the *New Atlantis* did ample justice to the new functions of our foundations for scientific research, and our specialized institutes and laboratories that utilize hundreds and even thousands of workers in what has increasingly become—with great rapidity since the national state itself became the main patron—factories for the mass production of knowledge, technologically exploitable and financially profitable.

In looking back over the fulfillment of Bacon's anticipations, it is plain that there were two critical points. The first occurred in the first half of the nineteenth century, when for the first time purely theoretic researches in physics, by Volta, Ohm, Henry, and Faraday, resulted, almost within a generation, in the invention of the electric telegraph, the dynamo, the electric motor; and within two generations in the invention of the telephone, the electric lamp, the x-ray, and the wireless telegraph: all of these being inventions that were not merely impracticable but technically inconceivable until pure scientific research made them live possibilities. The methods that were so fruitful in mechanics and electronics were then applied, with growing success, in or-

ganic chemistry and biology; though significantly enough the parts of technology with the longest accumulation of purely empiric knowledge, like mining and metallurgy, remained almost impervious to the advances of science.

The second critical point came during the first half of the twentieth century, along with a change of scale and magnitude partly brought about, almost automatically, by the expansion of the facilities for communication and the exploitation of new sources of power. This change lifted hitherto inviolable limits on human activities: a shot could be heard around the world by means of radio more than eleven times faster than it could be heard by the unaided ear a mile away. At this point, science itself became the technology of technologies; and as the mass production of scientific knowledge went hand in hand with the mass production of inventions and products derived from science, the scientist came to have a new status in society, equivalent to that earlier occupied by the captains of industry. He, too, was engaged in mass production.

The old image of the self-directed scientist still remains popular, particularly among scientists; but as science expands as a mass technology, the scientist himself becomes a servant of corporate organizations intent on enlarging the bounds of empire—by no means always "humane empire!" —and endowing themselves by means of invention with power and riches and worldly prestige. By this transformation the scientist has forfeited the qualities that were exalted, in the seventeenth century, as the very hallmark of the scientist—his detachment from worldly gains and his disinterested pursuit of truth. To the extent that his capacity for pursuing truth depends upon costly apparatus, collective collaboration, and heavy financial contributions from government or industry, he has lost, as Sir Charles Snow pointed out the other day, the capacity to stand alone and to say No—even on matters like the mal-exploitation of nuclear energy that threatens the future of the human race.

Not merely have the sciences, then, become technologies, but the scientist himself, caught in the corporate process, is fast becoming the model of a docile, standardized, organization man, imprisoned by his own obsolete premises, incapable of making his escape without altering those premises. I hope I need not underline the moral that Snow properly drew from this. But there is a corollary that I would stress. Since science as technology has already submitted, often with great eagerness, to political and economic pressures, for the sake of the immense scientific opportunities offered, it cannot escape facing the consequences of this submission, and actively helping to rectify them. The scientist now has the obligation of erecting intellectual and social safeguards against the frequently malign consequences of scientific discoveries, even if the creation of these internal checks and balances slow up, or occasionally bring to a halt, the process of scientific investigation or technological application. As an agent of technology, science no longer has the immunities or the irresponsibilities that it claimed for itself during its great quarrels with the Church. Today, the greatest danger to science comes not from the hostility of traditional institutions but from the patronage of contemporary ones.

Now if the fulfillment of Bacon's dream deserves our respectful recognition of his prophetic insights, it also imposes upon us a special duty—that of dissociating ourselves from the mythology he so largely helped to promote, so as to appraise, in the light of historic experience, his unexamined premises. These premises are now so thoroughly institutionalized that most of our contemporaries continue to act upon them without even a quiver of doubt. But observe: science as technology presents a series of problems that science, as the disinterested examination of nature in search of rational understanding, never confronted; for already it shows the same deep irrationalities and absurdities that mass production in other fields has brought about. The chief premise common to both technology and science is the notion that there are no desirable limits to the increase of knowledge, of material goods, of environmental control; that quantitative productivity is an end in itself, and that every means should be used to expand the facilities for quantitative expansion and production.

This was a defensible position in the seventeenth century when an economy of scarcity still prevailed everywhere. Then, each new facility for production, each fresh increment of energy and goods, each new scientific observation or experiment, was needed to make up the terrible deficiencies in consumable goods and verifiable knowledge. But today our situation is precisely the opposite of this. Because of the magnificent, awe-inspiring success of the sciences in widening the domain of prediction and control, in penetrat-

ing the hitherto inviolable mysteries of nature, in augmenting human power on every plane, we face a new predicament derived from this very economy of abundance: that of starvation in the midst of plenty. The quantitative overproduction of both material and intellectual goods poses, immediately for the Western World, ultimately for all mankind, a new problem: the problem of regulation, distribution, assimilation, integration, teleological direction. As science approximates more closely the condition of technology, it must concern itself with the machine technology's great weakness: the defects of a system that, unlike organic systems, has no built-in method of controlling its growth or modulating the enormous energy it commands in order to maintain, as in any living organism, a dynamic equilibrium, favorable to life. No one questions the immense benefits already conferred in many departments by science's efficient methodology: but what one must challenge is the value of a system so detached from other human needs and human purposes that the process itself goes on automatically without any visible goal except that of keeping the corporate apparatus itself in a state of productivity.

In science as well as in industry huge stockpiles have been accumulating which, on our present terms, cannot be adequately distributed or effectively used. There are even signs of a kind of crude valorization, with the destruction of older accumulations, through indifference or relegation to dead storage, in order to make room for current production and ensure its marketability. Our society has already reached the paradoxical state wherein our massive additions to the corpus of scientific knowledge have, through mere quantitative excess, lowered our capacity to make rational use of any part of it. In the exploding universe of science, the scattered parts are traveling at an accelerated rate ever farther from the human center. Because of our concentration on speed and productivity, we have ignored the need for integration and assimilation. The dubious morals of an acquisitive society have caught the once-disinterested promoters of science, along with their strange irrational compulsions. In practice this results in an inability to use more than a small fragment of the existing corpus of knowledge—namely that which is fashionable or immediately available, because it is being commercially exploited. This has already worked havoc in medicine, as any honest physician will tell you, and the results are visible in every other professional activity.

We are now faced, accordingly, as both Norbert Wiener and I have pointed out more than once, with the situation Goethe foresaw in the fable of the Sorcerer's Apprentice: we have achieved the magic formula for automatically increasing the supply of scientific knowledge; but we have forgotten the Master Magician's formula for regulating or halting the flood, and so are on the point of drowning in it. Science as technology gets its main financial support, and therefore its overall direction, from the national government, or from great industrial corporations like those engaged in exploiting new pharmaceutical preparations, chemical pest controls, or atomic energy, and from quasi-public philanthropic foundations exerting almost equally large powers. Though the professed aim of these organizations is truth and human welfare, they are governed in perhaps an ever greater degree by the Baconian goals of riches and power. On these premises they have no concern with ordering science in accordance with some human measure, toward the fulfillment of broader human goals: for this means altering the method of mass production and slowing down the whole process. Our schools and universities are helpless to restore an organic balance, because they themselves have accepted the same ideology and rely for a large part of their activities upon endowments that are scaled to the prospects of continued expansion and quick turnover: indeed the very possibilities for professional promotion depend more upon the number of scientific papers published than upon long-term results that may not be visible for a generation or more.

Is it not time, then, that we began to ask ourselves certain questions about science as technology that Bacon, by reason of his historic position, was too uninformed to put to himself? Are we sure that the control of all natural processes by science and techniques is by itself an effective way of relieving or improving man's estate? Is it not possible to have a surfeit of knowledge no less that a surfeit of food—with similar distress to and derangement of the organism? Have we not already evidence to show that science as technology may, through its inordinate growth, become increasingly irrelevant to any human concerns whatever, except that of the technologist or the corporate enterprise: that, indeed, as in the form of nuclear or bacterial weapons, it may

be not merely coldly indifferent but positively hostile to human welfare?

But I would go further. By what rational canon do we seek, on purely Baconian premises, to save time, shrink space, augment power, multiply goods, overthrow organic norms, and displace organisms with mechanisms that simulate them or vastly magnify some single function they perform? All these imperatives, which have become the very groundwork of science as technology in our technological society, seem axiomatic and absolute only because they remain unexamined. What rational purposes have ordained these objectives and provided us with a mechanism that has no means of self-regulation, no method of control except acceleration, and no goal except its own increase in power and authority? Is it from the standpoint of man or the machine, of the whole community's welfare or that of the corporate organization, that we accept these commitments, or fondly submit to these compulsions? Faster and faster, bigger and bigger, richer and richer, more and more inventions, more and more research—these are not rational imperatives at all. The fact that they have become embedded in our present conceptions of truth, value, and human progress gives them no scientific validation: indeed, the biological study of organisms suggests that this one-sided commitment to expansion is, in terms of life, a suicidal process.

Just because science as technology has begun to dominate every other aspect of science, we are bound, if only in self-preservation, to correct the mistakes Bacon unwittingly fostered or sanctioned. Science now makes all things possible, as Bacon believed: but it does not thereby make all things desirable. A good technology, firmly related to human needs, cannot be one that has a maximum productivity as its supreme goal: it must rather, as in an organic system, seek to provide the right quantity of the right quality at the right time and the right place for the right purpose. To this end deliberate regulation and self direction, in order to ensure continued growth and creativity, must govern our plans in the future, as indefinite expansion and multiplication have done during the last few centuries. The center of gravity is not the corporate organization, but the human personality, utilizing knowledge, not for the increase of power and riches, or even for the further increase of knowledge, but using it, like power and riches, for the enhancement of life. On these terms it may be that all the work that has been turned out by Solomon's Houses these last four hundred years will have to be done over again, or at least be revised and amplified and integrated and made humanly more adequate in order to do justice to all the dimensions of life.

The greatest contribution of science, the most desirable of all its many gifts, far surpassing its purely material benefits, has been its transformation of the human consciousness, through its widening illumination of the entire cosmic and historic process, and its transfer to man of the power to participate, with his whole being, in that process. Has the time not come, then—in technology as in every other aspect of the common life—to re-examine our accepted axioms and practical imperatives and to release science itself from the humanly impoverished and underdimensioned mythology of power that Francis Bacon helped to promote?

THE HISTORY OF SCIENCE IN AMERICAN UNIVERSITIES*

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AFTER the morning session in Philosophical Hall, those professors of the history of science present were invited to meet in the Library of the American Philosophical Society for an informal discussion of common interests. The chief subject posed for their consideration was the manner in which teaching in this field could best be organized on both the undergraduate and graduate levels.

There has been considerable variation in this regard in different universities; for example, the history of science is presented at Cornell by members of the history department, while at Wisconsin it is the province of an independent department, and at Princeton is handled by an interdepartmental committee. Again, at the Johns Hopkins University, the history of medicine is represented by its own department in the School of Medicine; whereas, at Yale University, both this discipline and the history of science as a whole are the common concern of a single, departmental group. It was thought that an exchange of views on such matters would be enlightening, and the following scholars were therefore asked to comment on arrangements in their respective institutions: I. Bernard Cohen (Harvard), Marshall Clagett (Wisconsin), Charles Gillispie (Princeton), Derek Price (Yale), and Owsei Temkin (Johns Hopkins).

A lively discussion ensued, in which others present—for example, Provost Loren C. Eiseley of the University of Pennsylvania, Dean Roy F. Nichols of the same institution, and Professor Harry Woolf, Editor of *Isis*—also participated. Both agreements and disagreements emerged in regard to such issues as (1) the respective roles of scientists, of general historians, and of historians of science in this area; (2) whether the field should be made a specialty within general history or a discipline in its own right; and (3) whether teaching programs should be limited to training historians of science, or should include the education of such other personnel as "science writers" or journalists.

There was no expectation that any of these matters would be settled; indeed, it was not even clear that uniformity among all universities would be desirable. But it seemed to be the general concensus that issues and alternatives were clearly stated, and that such clarity was desirable in an era when the teaching of the history of science is expanding so rapidly. Although the discussions will not be published, transcripts of tape recordings were prepared through the courtesy of Dean Nichols and have been distributed to participants.

* Contribution to the Conference Commemorating the 400th Anniversary of the Birth of Francis Bacon, sponsored by the American Philosophical Society and the University of Pennsylvania, January 23-24, 1961.

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